STATE OF MONTANA
NATURAL RESOURCE DAMAGE PROGRAM

REVIEW OF TRIANGLE ECONOMIC RESEARCH REPORT ON ECONOMIC LOSSES ASSOCIATED WITH RECREATIONAL FISHING IN THE UPPER CLARK FORK BASIN

PREPARED BY:
W. MICHAEL HANEMANN Ph.D.

OCTOBER 1995

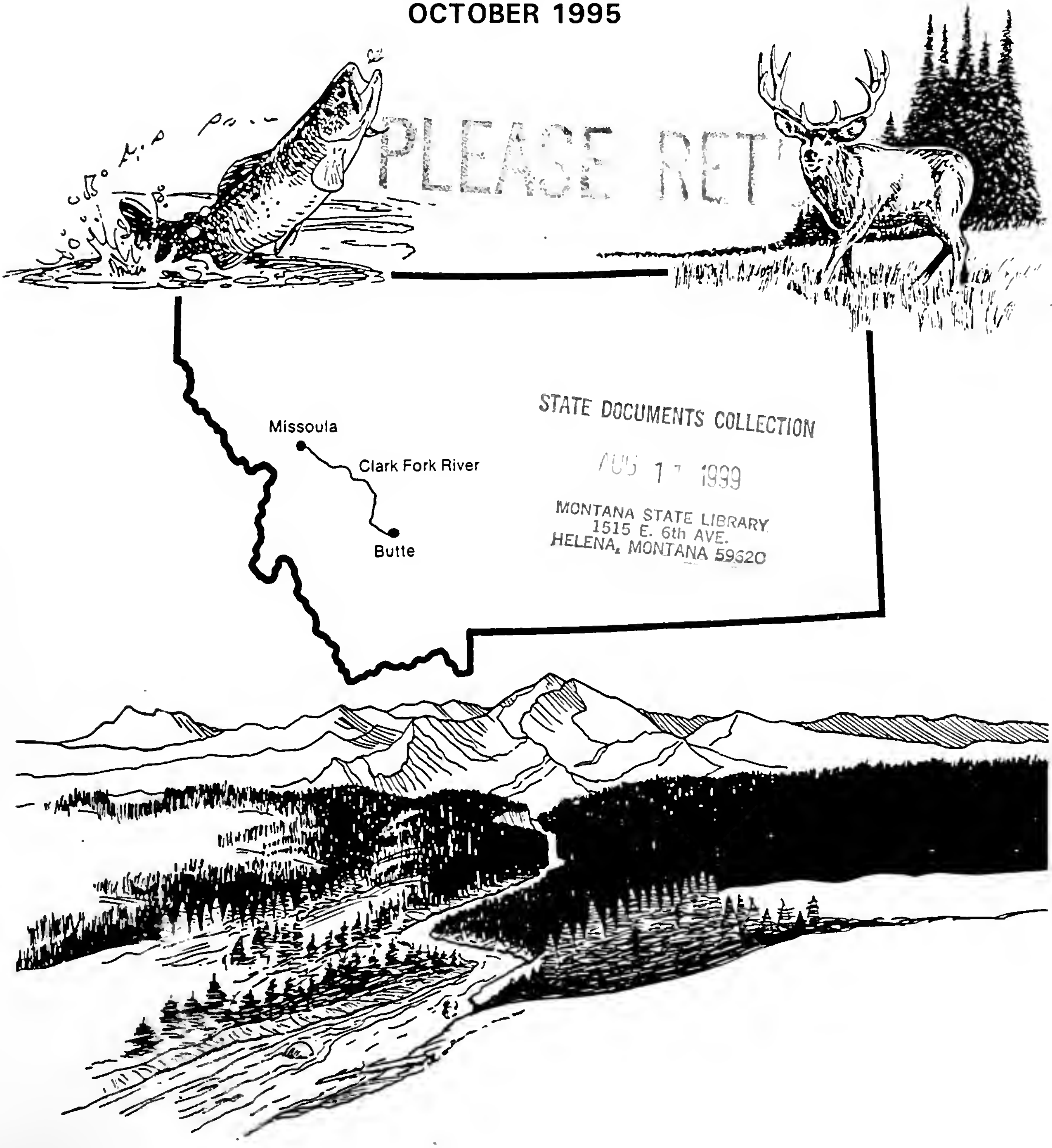


TABLE OF CONTENTS

REVIEW OF TRIANGLE ECONOMIC RESEARCH REPORT ON ECONOMIC LOSS TO RECREATIONAL FISHING IN THE UPPER CLARK FORK BASIN

by

W. Michael Hanemann, Ph.D.

October 27, 1995

TABLE OF CONTENTS

1. Personal Background and Qualifications

I am a Professor in the Department of Agricultural & Resource Economics at the University of California, Berkeley. I received a B.A. in Philosophy, Politics and Economics at Oxford University in 1965, an M.Sc(Econ) in Economics from the London School of Economics in 1967, and a Ph.D in economics from Harvard University in 1978. My dissertation, funded by the EPA, analyzed the recreation benefits associated with the abatement of water pollution in Boston Harbor.

Since joining the faculty at Berkeley in 1976. I have taught and conducted research in the fields of environmental economics and resource economics, with special emphasis on cost-benefit analysis and what is known as non-market valuation. I have published articles and reports in this field and I have conducted many academic and consulting studies.

Since 1988, I have been a consultant to the California Attorney General's Office and NOAA on various natural resource damage cases in California. I served as a consultant to the Alaska Department of Law on its suit for natural resource damages following the *Exxon Valdez* oil spill. I have conducted studies on the economic value of sportfishing for the Alaska Department of Fish & Game, on the economic value of wetland preservation for the Inter-Agency San Joaquin Valley Drainage Program, and on the public trust values associated with Mono Lake for the California State Water Resource Control Board's EIR in connection with its recent Mono Lake Water Rights Decision. Earlier, I served as the Board's principal economic advisor for the first three years of its Hearings on Water Diversions from the San Francisco Bay/Delta.

Cases in which I have testified or been deposed are listed in an Appendix.

My resume is attached as another Appendix.

2. Overview of this Report

I was asked to review a report entitled "Volume III: Report on Potential Economic Losses Associated with Recreation Services in the Upper Clark Fork River Basin" by William H. Desvousges and Steven M. Waters of Triangle Economic Research, Durham NC, dated July 13, 1995 ("the TER Report"). This is a study of the economic loss to anglers in Montana resulting from hazardous releases in Upper Clark Fork River Basin. The TER Report was accompanied by Appendices A - D, plus several related documents, as follows:

Volume II: Critique of the State of Montana's Recreation Study. Expert Report of William H. Desvousges and Steven M. Waters, July 13, 1995.

Expert Report of Daniel L. McFadden, July 12, 1995.

Research Triangle Institute, Compensable Value Damages: ARCO Response to State of

Montana Damage Assessment Report Upper Clark Fork River Basin. Volume II ARCO's Compensable Value Damage Estimates. January 1994

Research Triangle Institute, Compensable Value Damages: ARCO Response to State of Montana Damage Assessment Report Upper Clark Fork River Basin. Appendix A: Random Utility Models. Appendix B: Telephone Survey, January 1994

Research Triangle Institute, Compensable Value Damages: ARCO Response to State of Montana Damage Assessment Report Upper Clark Fork River Basin. Appendix D: Mail Survey Results, January 1994

RCG/Hagler Bailly, Assessment of Damages to Anglers and Other Recreators from Injuries to the Upper Clark Fork River Basin, January 1995

RCG/Hagler Bailly, Compensable Natural Resource Damage Determination, Upper Clark Fork River NPL Sites, January 1995.

Deposition of Janet Decker-Hess, Volumes I & II, March 21 & 22, 1995.

In addition to these documents, I was given floppy disks with various computer files from Triangle Economic Research (TER), as well as various internal memoranda and papers by TER staff and consultants that had been obtained through the discovery process. I subsequently obtained several reports from the Montana Department of Fish, Wildlife and Parks (DFWP) which will be mentioned below.

I have not seen the complete set of data collected by TER in the course of preparing its analysis of economic loss to recreation in the Upper Clark Fork River Basin. What I have seen represents data that were used in the final analysis for the TER Report plus a portion of the earlier data. Likewise, I have seen some, but by no means all, of the computer programs used by TER in the course of its analysis. The observations here represent my preliminary assessment based on the information that I have received so far. I reserve the right to modify this report in any respect as I receive new information.

My review of TER's estimate of the economic loss to recreation associated with natural resource injuries in the Upper Clark Fork River Basin consists of four components. In Section 3, I review the surveys that provided much of the data for the analysis in the TER Report. In Section 4, I review the variables used in TER's statistical model of sport fishing. In Section 5, I review the structure and estimation of the model. In Section 6, I review the interpretation of the model results and their extrapolation to the estimation of aggregate recreation losses in the Upper Clark Fork River Basin.

In forming my opinions, I have been assisted by my graduate student Craig Mohn and his wife, Mariko Tanaka. Craig carried out computer analyses under my direction. Mariko reviewed maps and books and compiled a map of the TER sites under my direction. I wrote this report

and it represents my own opinions.

In addition to the reports mentioned above, and other items to be mentioned below, for general information about fishing in Montana I consulted the following books:

Delorme Mapping, *Montana Atlas & Gazetteer,* 1994.

Hank Fischer, *The Floater's Guide to Montana* Helena: Falcon Press Publishing Co, 1986.

Chuck Fothergill and Bob Sterling, *The Montana Angling Guide* Woody Creek: Stream Stalker Publishing, 1988.

John Holt, *Montana Fly Fishing Guide, Volume 1: West of the Continental Divide* Helena: Greycliff Publishing Co, 1995.

Dick Konizeski, *The Montanans' Fishing Guide. Volume I: Montana Waters West of the Continental Divide* Missoula: Mountain Press Publishing Company, 4th edition, 1992.

Dick Konizeski, *The Montanans' Fishing Guide. Volume II: Montana Waters East of Continental Divide* Revised and updated by James A Derleth. Missoula: Mountain Press Publishing Company, 4th edition, 1992.

Charles Meck and Greg Hoover, *Great Rivers - Great Hatches* Harrisburg: Stackpole Books, 1992.

W. C. McRae and Judy Jewell, *Montana Handbook* Chico: Moon Publications, Second edition, 1994.

3. The MOR Survey

3.1 Introduction

In any empirical research, the data are crucial. Even the most dazzling theoretical model will come to naught if it is applied to unreliable, unrepresentative or contaminated data that cannot be cleansed of these defects. It is appropriate, therefore, to start my review of TER's analysis of recreation losses by considering the data on which it is based. These data come from a special survey conducted by the Research Triangle Institute (RTI) which it named the Montana Outdoor Recreation (MOR) Survey. The MOR survey was conducted in two phases. The first phase, conducted in July 1992, was a screening interview in which a random sample of individuals in the state of Montana were contacted by telephone and asked whether anybody in their household aged 16 or over participated in outdoor recreation. If some members of the household did participate in recreation, the interviewer selected one of them to serve as the respondent for the survey. The interviewer asked the respondent first for his/her opinions about outdoor recreation in the state of Montana, and then whether he/she had participated in 7 specific outdoor recreation activities during the past 12 months, and how much. Next, respondents were asked whether they planned to participate in any of those activities over the *next* 12 months, and whether they planned to live in Montana during that time. If they answered "yes" to both questions, they were asked if they would volunteer to join a panel of people who would be paid $35 to record brief summaries of all their visits to recreation sites over the next year. The interview concluded with a series of questions about demographic and economic variables.

The second, panel phase of the MOR Survey was for those respondents to the telephone survey who had agreed to participate in the recreation panel. Seven times over the next 14 months they were mailed a $5 check together with a trip summary booklet covering a two-month period (wave), starting with July/August 1992 and continuing through July/August 1993. They were asked to record in the booklet information on all recreation trips during the two-month period. For this purpose, the booklet was divided into 9 sections: fishing, boating and water sports, camping, hiking and trail activities, game hunting, waterfowl hunting, recreational trapping, other activities, and multiple destination trips. A large map was included along with each booklet, and respondents were requested to place a numbered sticker on the map to mark the precise destination of each trip. The booklets were to be returned in a pre-paid envelope at the end of the two-month period. Panel members who did not return their booklets on time were sent reminder postcards, which included a toll-free phone number to call with any questions. Four to six weeks following the postcard mailing, follow-up telephone calls were made to all panel members who still had not returned books. A comprehensive data management system was established at RTI to track the progress of the survey and ensure quality control, covering mailing of booklets, receiving of booklets, editing of booklets, and keying of data.

Taken together, the two phases of the MOR Survey constitute a very substantial survey effort. I have been conducting recreation and other surveys since 1974, and I am not aware of any private recreation survey that was so extensive or elaborate. Having a survey organization like RTI conduct a 7-wave panel for a statewide population is an extremely expensive

undertaking. If one can afford the cost, and if it is done correctly, a panel survey offers important advantages. Using a random sample of all the households in the area of interest is a more straightforward approach to sampling than conducting an intercept survey of recreationists on-site. It is much more costly since one needs a substantially larger sample to get the same information about recreation activities but, in principle, it can avoid a variety of problems and confounding factors that otherwise need to be adjusted for, such as the over-sampling of avid recreationists. Likewise, the panel approach in which respondents keep a "diary" of their activities provides more complete and accurate information; as long as they continue to participate in the survey, it allows one to track respondents' behavior over the entire season or year.

However, it is important to recognize that the outcomes of surveys can sometimes differ from these ideals. Sampling people is inevitably far more complicated than sampling inanimate objects. People may not be willing to participate in a survey, they may wander off, or they may lose interest half way through. And, if this happens, it may not happen at random. The people who do this may be systematically different from those who don't, but not necessarily in ways that can readily be identified and measured by the analyst. Real-world sampling sometimes produces outcomes that are not what one expects. Therefore, when one assesses the MOR Survey, it is important to look at not only the amount of effort that went into the survey but also the nature and quality of the output -- how well did the survey perform in practice, and what did it produce? To answer these questions, I did two things: I examined the data from the MOR Survey, and I checked it against information that is available from other sources.

In addition to the MOR Survey and the angler survey conducted by Hagler Bailly (1995) for the State of Montana, there are at least two other major sources of information about outdoor recreation in Montana -- the 1991 National Survey of Fishing, Hunting, and Wildlife-Associated Recreation (FHWAR), conducted by the U.S. Bureau of the Census for the U.S. Department of Interior's Fish and Wildlife Service (U.S. Department of the Interior, 1993); and statewide mail surveys of angling pressure conducted by the Montana Department of Fish, Wildlife and Parks in 1989, 1991 and 1993. Brief descriptions of them follow.

The 1991 National Survey of FHWAR was the eighth such survey conducted since 1955 with the purpose of gathering information about the activities of hunters, anglers and nonconsumptive participants; the information is regarded as an important tool by natural resource managers and industry planners. The 1991 survey was conducted in two phases. The first, screening phase, conducted in January and February 1991, interviewed a sample of 129,500 households nationwide, primarily by telephone, to determine who in the household had fished, hunted or engaged in nonconsumptive, wildlife-related activity during 1990, and who planned to engage in those activities in 1991. In Montana in this phase, about 1,070 completed household interviews were obtained, for a response rate of approximately 93.6%. Roughly 68% of the interviewed households were contacted by telephone; the remainder were contacted by personal visit. The second, more in-depth phase covered recreation during 1991. This phase involved three detailed interviews conducted every four months with samples of likely anglers, hunters and nonconsumptive participants who were identified in the initial screening. These interviews were conducted mainly by telephone, with in-person interviews for those respondents who could not

be reached by telephone. Respondents in this phase were limited to those at least 16 years old. Each respondent provided information pertaining only to his/her activities. Sample sizes were designed to provide statistically reliable results at the State level for fishing, hunting, and nonconsumptive activities. In Montana, about 1220 persons were designated for interviews in the second phase of the survey and about 1180 of these completed interviews, for a response rate of 96.9%. It is the information from these interviews in the second phase that is reported in U.S. Department of the Interior (1993).

The Montana Department of Fish, Wildlife and Parks has conducted statewide angling mail surveys from time to time in the past. In 1989, the Montana Legislature approved funding for an "Enhanced Survey of Angling Pressure" to be conducted every other year. Since then, surveys have been conducted in 1989, 1991, and 1993 using essentially the same format. The results of the 1993 survey are still in press. For my analysis, therefore, I focus on the 1991 survey (McFarland and Hughes 1994). The survey involved mailing questionnaires to a stratified random sample of resident and nonresident anglers each month covering the 12-month period from March 1991 to February 1992. Respondents were asked whether they had fished in Montana during the previous month and, if so, they were requested to list each water that they had fished, the date, the number of days of fishing, and the mode of fishing. The samples were drawn from holders of resident and nonresident fishing licenses. For the residents and some of the nonresidents, fresh samples were drawn monthly as records of fishing license sales arrived in Helena. Nonresidents with 2-day licenses were sampled once, in February, about their fishing for the entire license year. Over the 12 months of the survey, questionnaires were mailed to 79,120 residents and 16,230 non-residents, of which 71,017 were successfully delivered to residents and 14,205 to nonresidents. Responses were received from 45,384 residents and 8,764 nonresidents, representing response rates of 63.9% and 61.7%, respectively.

The remainder of this section of my report is organized as follows. In Section 3.2 I review the first phase of the MOR survey, the screener survey of a random sample of Montana residential telephone numbers. In Section 3.3 I discuss the recruitment of people identified as eligible in the telephone survey to participate in the 14-month panel survey. In Section 3.4 I review the panel phase of the MOR survey. My conclusions are summarized in Section 3.5.

3.2 Telephone Phase of the MOR Survey

In its general structure, RTI's MOR survey is rather similar to the Census Bureau's National Survey of FHWAR: both involved a panel of individuals who reported on outdoor recreation in Montana, in one case during calendar 1991 and, in the other, during the period July 1992 - August 1993. In both cases, the panel was recruited through a preliminary, screener survey of the general population. The MOR survey involved telephone interviews with a sample of 2,071 households, obtained via random digit dialing, representing a response rate of 73%; the FHWAR survey involved telephone or in-person interviews with a random sample of 1,070 households, selected from samples used for the Census Bureau's Current Population Survey, representing a response rate of 93.6%. In each case, the chief purpose of the screener survey was to pave the

way for the subsequent panel survey by identifying the target population of interest, which was approximately the same in both surveys -- the population of Montana residents aged 16 and older who planned to engage in outdoor recreation during the period of the panel survey. These were mainly people who had participated in those same activities -- in the sense of having taken at least one such outdoor recreation trip -- during the period covered by the screener interview, namely calendar 1991 for the Census Bureau and July 1991 - June 1992 for the MOR survey. Hence, the screener survey is a guarantor of the integrity of the panel survey. If it generates a sample that is fully representative of the target population, this augurs well for the accuracy and reliability of the panel survey. The converse is true if it generates a sample that poorly represents the target population.

As indicated, the MOR screener survey employed a larger sample than the FHWAR screener survey, but it also had a lower response rate. Another difference was survey mode -- the MOR screener was conducted entirely by telephone, while the Census Bureau used in-person interviews for almost one third of its sample. Professor McFadden has mentioned two features of the MOR telephone survey that cause him some minor concern. The first is that telephone surveys may not have provided a fully representative sample. By definition, households without telephones were not included in the MOR sample, and households with multiple telephone lines were more likely to be sampled than those with but one line. Similarly, households who are not at home were much less likely to be contacted and included in the MOR sample than households who stay at home more. Secondly, Professor McFadden notes that the screening of household members for the panel could have introduced a degree of arbitrariness in the MOR sample. The probability of being selected depended on the number of adult recreators in a household. And the adult recreators within the household who were at home at the time of the screening call tended to be included more readily than those who were not. Considerations such as these may have led the Census Bureau to use in-person interviews for the sake of a more representative sample in its FHWAR survey.

Professor McFadden states that the sampling procedure employed by RTI was sufficiently straightforward that appropriate weights can be, and were, developed such that the weighted sample in the MOR survey is representative of the target population. To see whether this is so, one should look at the data. The TER Report uses the weights to extrapolate from the MOR telephone sample of 2071 respondents to the entire adult population of Montana. In Table A-10, it provides estimates of the number of residents of Montana aged 16 and over who took one or more outdoor trips for each of the seven recreation activities covered by the survey during the 12 months prior to the telephone survey. These would constitute the target population for the subsequent MOR panel survey. Thus, for example, the TER Report estimates that 326,046 adult Montana residents participated in fishing in 1991/92. In producing these estimates, the TER Report assumed a total adult population in Montana of 583,914 (Table A-13). In fact, according to the Census Bureau, in 1991 there were about 600,900 persons aged 16 and over in the state of Montana. The difference is small -- about 2.9% -- but it implies that the TER estimates of participation should be increased by something like that proportion to benchmark them against the actual 1991 population. This presumably would push up the TER estimate of adult anglers residing in Montana in 1991 from 326,046 to something like 335,500.

As I noted earlier, there are two other sources of information about participation in outdoor recreation by Montana residents in 1991, the Census Bureau's National Survey of FHWAR and the State of Montana Department of Fish, Wildlife and Parks (DFWP) statewide mail survey of angling pressure. The FHWAR survey covers fishing, hunting, and non-consumptive wildlife-associated recreation.[1] The Montana DFWP survey covers just fishing. For fishing, the Census Bureau's estimate of the number of adult anglers in the state of Montana in 1991 is 170,800, with a 90% confidence interval of about 154,300 to 187,300 (see Table 1). Even if one takes the upper end of range, this is substantially less than TER's estimate of the same target population. Relative to the Census Bureau's finding, TER overestimates the number of adult anglers in Montana in 1991 by 74% if one does not adjust for its underestimate of the state population, and by about 79% if one does. The Montana DFWP also overestimates the number of adult anglers in Montana in 1991, but to a much smaller degree. Its estimate of 218,567 adult resident anglers is 17% larger than the upper end of the Census Bureau's range.

On the basis of these data, I would be inclined to put the number of adult resident anglers in Montana in 1991 at around the upper end of the Census Bureau's range, namely about 187,000. I am not necessarily surprised that the Montana DFWP survey produces a slightly larger total, because it is based on counts of license holders. There will always be some individuals who purchase fishing licenses because they hope or intend to go fishing during the coming season but then other things intervene -- they are too busy, they become sick, they move away, etc -- and they never do get to go fishing. Therefore, I would expect the number of license holders to exceed the number of actual anglers (defined as people who make at least one fishing trip during the year) by at least some percentage. However, this does *not* apply to surveys such as the MOR survey which are based on the general population rather than a sample frame of license holders. The substantial error in TER's estimate of anglers in Montana is, therefore, all the more surprising.

Hunting is another activity where one can independently verify the TER Report's estimate of a target population for the MOR panel survey. The FHWAR survey provides an estimate of the number of adult residents who in 1991 participated in some form of hunting and, separately, in big game hunting, small game hunting, migratory bird hunting, and hunting for other animals. The TER Report provides separate weighted population estimates for waterfowl hunting and game hunting. From the definitions provided, the Census Bureau's "migratory bird" category appears to coincide with RTI's "waterfowl" category. Therefore, these two estimates of participation should be comparable. As shown in Table 1, TER estimates that about 114,900 adult residents of Montana participated in waterfowl hunting in 1991/92. The Census Bureau's estimate is 13,500 (it gives no standard error or confidence interval in this case). The difference is quite striking. Relative to the Census Bureau's finding, TER overestimates the number of adult waterfowl hunters in Montana in 1991 by about 751% if one does not adjust for its underestimate of the

---

[1] The last is defined as observing, photographing or feeding fish or other wildlife, excluding trips to zoos, circuses, aquariums, and museums.

TABLE 1: NUMBER (in thousands) OF MONTANA RESIDENTS AGED 16 AND OLDER PARTICIPATING IN FISHING & HUNTING

ACTIVITY	1992 RTI PHONE SURVEY (1)	1991 CENSUS BUREAU FHWAR SURVEY (2)	1991 STATEWIDE ANGLING PRESSURE SURVEY (3)
FISHING	326.0	170.8 +/- 16.5	218.6
ALL HUNTING	254.2**	157.6 +/- 14.8	
GAME HUNTING	246.5		
WATERFOWL HUNTING	114.9	13.5	

SOURCE: (1) TER (1995), Table A-10
(2) 1991 National Survey of FHWAR: Montana, Tables 2, D-1, D-2
(3) McFarland (1994a), Table 12
** Estimated from MOR data

state population, and by about 775% if one does.

Since the MOR telephone survey combines small and big game hunting into a single category, whereas the FHWAR survey keeps them separate, one cannot directly compare their findings. However, it is evident from Table 1 that, relative to the Census Bureau, the TER Report overestimates participation in game hunting. The TER Report estimates that about 246,500 adult residents of Montana participated in game hunting in 1991/92, while the Census Bureau estimates that only about 157,600 adult residents participated in *any* form of hunting in 1991, with a 90% confidence interval from 142,400 to 172,400. To obtain a more direct comparison between the surveys, I aggregated the MOR telephone survey's two categories of game hunting and waterfowl hunting. If anything, this should produce an underestimate of the Census Bureau's "hunting" category since it may omit some types of hunting covered by the Census Bureau. In the MOR survey, 908 of 2071 respondents reported having participated at least once in game hunting and/or waterfowl hunting during the past 12 months. At this time, I do not have the specific weights that TER used to extrapolate from the individuals in the MOR telephone survey to the entire adult population of Montana; these apparently were not part of the data that were provided to me. Based on what I have been able to deduce about TER's weights, I would estimate that the 908 respondents in the phone survey would extrapolate to about 254,240 hunters in the entire adult population of Montana. Relative to the upper bound of the Census Bureau's confidence interval for resident adult hunters, this would be an overestimate of about 47%.

In addition to participation, the MOR telephone survey generated information covering a variety of attitudinal, demographic, and economic variables pertaining to adult outdoor recreators in Montana -- their age, sex, income, education, whether they owned a boat, a cabin or a hunting lodge, what type of vehicle they used for recreation, etc. Age, income, and sex also happen to be variables reported in the National Survey on FHWAR. Unfortunately, the ranges that RTI used in the MOR telephone survey for recording age and income are different from those employed by the Census Bureau. However, a direct comparison is readily available for sex. The results are shown in Table 2. While males account for 49% of the population aged 16 and older in Montana, they account for a larger percentage of adult anglers and hunters because those activities are much more favored by males than females in that state. The Census Bureau reports that males account for 75% of the anglers and 87% of the hunters in Montana. By contrast, when extrapolated to the adult population of Montana, the MOR telephone survey implies that males account for 61% of the anglers and 70% of the hunters in the state. This is clearly inconsistent with the Census Bureau's findings.

In summary, there is a systematic pattern whereby the MOR telephone survey overestimates the size of the target populations of outdoor recreationists in Montana in 1991/92, sometimes to an enormous degree, and overstates the extent to which they are composed of females. In the light of this evidence, I conclude that the MOR telephone survey failed to meet its goal of providing a representative sample of the target population of outdoor recreationists in Montana. The magnitude of this failure is quite striking.

In the remainder of this section I discuss some possible reasons for the failure, and then

TABLE 2: SEX OF MONTANA RESIDENT ANGLERS AND HUNTERS, 1991

	1991 CENSUS BUREAU FHWAR SURVEY (1)	1992 RTI MOR SURVEY (2)
ANGLERS		
% Male	75.1%	61.0%
% Female	24.9%	39.0%
HUNTERS		
% Male	86.6%	70.0%
% Female	13.4%	30.0%

SOURCE: (1) 1991 National Survey of FHWAR: Montana, Table 13
(2) Estimated from MOR data

comment on what I see as its implications.

There are at least two factors that could have caused the errors in the MOR telephone survey. One is the phenomenon known in the survey research literature as *telescoping*, which is the tendency of respondents when recalling past events to relocate them to either an earlier or later period than when they actually occurred (Neter and Waksberg, 1964). The latter has often been found the more serious problem -- people report as happening within the last year, say, things that had actually happened earlier, leading to an exaggerated estimate of their frequency (Loftus et al., 1990). Thus, respondents who had gone fishing in the past, but not within the previous 12 months, might have erroneously reported that they had fished in the MOR telephone survey. Another explanation (these are not mutually exclusive) is that the survey might have mistakenly identified as anglers or hunters individuals who were neither. This is consistent with both the exaggerated numbers of anglers and hunters and also the exaggerated proportion of females. In effect, there are two populations: the population of anglers, hunters, etc, which is highly skewed to males, and the population of non-anglers and non-hunters, which in Montana is about 38% males and 62% females. The purpose of the telephone survey is to identify the first population but, by mistake, it also picks up some members of the second. This has two consequences: it produces an overestimate of the size of the target population, and one which is less heavily weighted to males. This can be thought of as a contamination phenomenon: the target population is contaminated with some members of a non-target population. I estimate that this contamination could account for an upward bias of about 106,000 (56%) in TER's estimate of adult resident anglers in Montana.

What could have brought about the contamination? It may have arisen as a result of errors in the design of the MOR telephone survey or carelessness in its execution. For example, wives may have been reporting on their husband's fishing, and this was erroneously recorded as though they were reporting on their own fishing. Or, wives who accompanied their husband on a fishing trip but didn't themselves fish might have said "yes" when the interviewer asked if they had participated in fishing. These are errors that can be caught with careful wording of the questionnaire and vigilance on the part of the interviewer.[2] It is possible that this was compounded by the exclusive use of telephone for the MOR survey. When the interviewer is present in person, as in a third of the Census Bureau's interviews, it is easier to monitor whether respondents are answering the question as intended, and to probe when they give incomplete or ambiguous responses.

Whatever the factors that caused it, I believe that the failure of the MOR telephone survey has serious implications. These concern: (1) the quality of the analysis that went into the TER Report; (2) the reliability of the weighting scheme employed by TER; and (3) the design of the

[2] The TER Report conveys the impression that the MOR survey was initiated with some haste. It mentions that there was a single training session for interviewers on the day before the telephone survey began, which consisted of 90 minutes of formal training followed by several hours of hands-on practice with the instrument. This does not appear to have left time to revise the interview script to incorporate feedback from the interviewer training. Inadequate interviewer training could certainly help explain the overcounting of female anglers and hunters.

subsequent MOR panel survey.

With regard to the first, it is both common sense that, when one measures something for which other researchers have also obtained measures, one checks one's own results against theirs and investigates any discrepancies. Not to have done this is an indictment of the quality of the research. In the case of the TER Report, the failure to reference the 1991 National Survey of FHWAR cannot have been due to lack of information. I know that members of the TER research team and their colleagues attended Annual Meetings of the W133 Environmental Economics Group at which representatives of the Fish and Wildlife Service made presentations about the 1991 National Survey and, on at least one occasion, freely distributed copies of the report. If not due to ignorance, the failure of TER to reference the National Survey must be attributed to either absent-mindedness or deliberate design. Either way, it is an egregious omission.

With respect to weighting, while there is general agreement about the desirability of using sampling weights when the aim is to estimate population-level statistics, two qualifications should be noted. First, weighting is not a panacea that corrects for all errors in survey execution. Second, the right weights have to be used. The MOR telephone survey illustrates the problem. The MOR telephone survey data were weighted to adjust for differences in sampling rates by region and to benchmark against known values for various population-level demographic variables, such as the age and sex distribution of adult Montana residents. With these weights, therefore, the 2071 respondents blow up correctly to Montana population -- for example, the weighted ratio of males to females for the full sample of respondents matches the true ratio for adult Montana residents. But, as we have seen, the weighted sample fails miserably at representing the population of anglers or hunters in Montana. If the aim was to represent outdoor recreators in Montana -- which clearly *was* the target population for recruiting the panel in the next phase of the survey -- the wrong weights were being used. Too many people, and especially too many women, are wrongly identified as being anglers or hunters. Whether this occurred because of telescoping or contamination, the weighting is inadequate to undo errors of survey execution.[3]

Finally, as I show below, the failure of the MOR telephone survey to produce an accurate sample of the target population of outdoor recreators in Montana had unforeseen repercussions on the effectiveness of the panel survey which followed it. Because of the failure of the telephone survey, the TER research team had an exaggerated impression of the number of anglers and hunters in the Montana population. Therefore, they overestimated the number of anglers and

[3] This is not an isolated occurrence. Professors Hausman and McFadden ran into the same problem in their study for Exxon of the impact of the *Exxon Valdez* oil spill on outdoor recreation by residents of Alaska (Hausman, Leonard and McFadden, 1993). That study also involved a two-phase survey, the first being a screening survey of a random sample of Alaskan phone numbers to identify households which had participated in various types of outdoor recreation in Alaska in 1988 and 1989, and the second being a follow-up mail survey of respondents identifying themselves as recreational participants. On the basis of their surveys, Professors Hausman and McFadden form an estimate of fishing in Alaska by residents which substantially overstates the amount of fishing relative to the estimates of the National Survey of FHWAR and other sources. Too many respondents may have wrongly identified themselves as having participated in fishing in Alaska in 1988 and 1989, and this was not adequately corrected by the weighting scheme that Professors Hausman and McFadden employed.

hunters in the panel that they recruited from among the respondents to the telephone survey. Therefore, their panel turned out to be less informative than they had expected. Given the true proportion of anglers and hunters, their panel was too small for what they wanted it to accomplish.

3.3 The Recruitment of the MOR Panel

At the end of the MOR telephone survey, respondents were asked whether they planned to participate in any of 7 outdoor recreation activities over the next 12 months, and whether they planned to live in Montana during that time. If they answered "yes" to both questions, they were asked if they would volunteer to join a panel of people who would be paid to record their recreation activities over the next year. Of the 2071 respondents, 1529 were eligible to join the panel because they met both criteria.[4] Of these eligibles, 1134 reported that they had participated in fishing during the previous year, 282 reported that they had not participated in fishing, and 113 said they did not know whether they had participated in fishing. I will treat the last group as *not* having participated. Of the eligible recreators, 1149 agreed to participate in the panel survey; 881 had participated in fishing during the previous year, while 268 had not. Thus, anglers were more likely (78%) to join the panel than non-fishing recreators (68%), a difference which is statistically significant. Hence, anglers are over-represented in the panel relative to other outdoor recreators. This is a flaw in a survey intended as a representative sample of outdoor recreators in Montana.

3.4 The MOR Panel Survey

Panel, or longitudinal, surveys are recognized by survey researchers as advantageous for measuring change or for measuring activities that occur at varying rates over time such that measurement at a single point in time would produce unreliable or unrepresentative information. Although dating back to before World War II, the use of panel surveys in the social sciences greatly expanded in the late 1960's and early 1970's, and they came to receive considerable attention from professional and academic researchers (Duncan, Juster and Morgan, 1986). While panel surveys can offer considerable advantages in providing accurate information on time-varying phenomena, there are also some potential drawbacks. One is cost -- the expense of creating and maintaining a panel may can be quite substantial and may lie beyond the budget of most researchers. Another is the logistics of data management and file maintenance for large-scale panels. In addition, Sudman and Ferber (1979) mention problems of getting and maintaining cooperation and data accuracy:

"The first problem [getting and maintaining cooperation] is one that can seriously affect

[4] The TER Report says that 1532 telephone respondents were eligible for the panel. I am unable to replicate that number from the data provided to me. For these data, I find that 1535 telephone respondents planned both to live in Montana and to participate in outdoor recreation during the next year. However, 6 of them had addresses with zip codes that would make them nonresidents.

> the representativeness of a panel study. If panel members are to keep written records ... the initial rate of cooperation can be as low as 50% even by personal interview and may be much lower if cooperation is sought by mail. Those who cooperate are more likely to be more educated, in clerical or professional occupations, in the middle income levels, and in the younger and middle age brackets. As a result, a panel at the beginning of the operation may not be representative of the population from which it was selected. This is only the start of the problem, however. Dropouts can be substantial. About half of the people that even consent to participate in a panel study may drop out after the first two or three rounds, especially if they are asked to keep written record. As a result, a panel operation can become increasingly unrepresentative of the population from which it originally came...
>
> The accuracy of panel member reports is another problem, one that seems to vary with the type of study. This problem is most serious with diary studies, where panel members requested to record their purchases of many different items day by day in diaries that are turned in or mailed every week...As a result, the accuracy of such information can vary greatly within the same study by the type of product and the type of information that is being sought."

Another potential problem in panel surveys is what is called *conditioning* or *time-in-sample bias*. This is mentioned by Sudman and Ferber and is discussed at greater length by Bailar (1989), who defines it as "the phenomenon that estimates from people reporting for the same time period but with different levels of exposure to the survey have different expected values....Bias is observed when a significantly higher or lower level for a characteristic occurs on the first interview than in subsequent interviews, when one would expect the same level." This occurs, for example, in consumer expenditure diary surveys where there is more comprehensive reporting on the first day of the week than later in the week, and in the first week than later weeks. Bailar mentions several factors that could be responsible for such phenomena. Respondents might become trained by their exposure to the survey and provide more accurate data in later interviews. Respondents might actually change their behavior during the course of the survey. Interviewers might not provide the same stimulus to the respondents in the later interviews as in the first one. Respondents might learn that some responses mean additional questions, so that start avoiding to give certain answers. Respondents might become bored with the survey process and stop working as diligently or energetically at providing accurate responses in later interviews. Whatever the precise cause, Bailar regards time-in-sample bias as a major concern for panel surveys.

The existing literature on panel surveys deals mainly with items other than outdoor recreation but, in principle, the same issues could apply to panels of recreationists. It is important, therefore, to consider how effectively the MOR survey overcame the challenges to a successful panel survey. It certainly appears to have been a costly and well funded undertaking. How well did it overcome the other challenges?

My ability to answer this question is limited by the inadequacy of the information provided to me. The computer files that I have received with data from the MOR panel survey

contain mainly data used for estimating the fishing demand model presented in the TER Report. The model uses only a subset of the data collected in the MOR panel survey, and the files in my possession constitute only a portion of the data collected in the MOR panel survey.

In addition to computer files, I have written descriptions and analyses of data from the panel survey contained in the TER Report and in other documents and memos generated by TER staff and consultants that were obtained by the State of Montana and provided to me. These written materials provide an incomplete account of the panel survey and its findings. For example, the description of the results of the MOR panel survey in the TER Report is confined to the fishing portion of the survey and -- for reasons that are never explained -- entirely omits the other seven outdoor recreation activities that were covered in the survey. Moreover, the description of the cleaning of the fishing data in the TER Report omits several pertinent details, such as the number of respondents eliminated during the cleaning process.[5]

Furthermore, some of the information in the written materials conflicts with other written information or with information in the computer files. For example, I have an RTI Memorandum written by Sharon Snow to Sara Hudson and others on June 13, 1994 (TER000033384-89) on the subject of "Final data cleaning activities for Montana Outdoor Recreation Survey" which describes data cleaning for the last batches of survey data, consisting of survey booklets from the final wave of the survey (July - August, 1993), as well as late returns of survey booklets from earlier waves all the way back to the first wave (July - August, 1992). However, the number of fishing trips that remains after cleaning as reported in this memorandum is substantially different from the number of fishing trips used to estimate the TER model and contained in the computer file provided to me. There are several other differences in details of the cleaning process as described in the June 13 Memorandum versus the TER Report.

In short, at this time I do not have adequate access to the data generated by the MOR panel survey to fully verify or reproduce the analysis in the TER Report. Therefore, at this time I can provide only a partial assessment of the MOR panel survey.

The TER Report mentions that two of the 1149 people who agreed to participate in the MOR panel survey withdrew from it before the first booklet was mailed, and another 171 panel members either moved out of state or withdrew from the survey for a variety of reasons (including a few who said they were concerned about the sponsor of the survey). For the remaining participants, the TER Report states (p. 18) that the average response rate for returned booklets over the seven mailings between Wave 1 (July-August 1992) and Wave 7 (July-August 1993) was nearly 73%. Since I do not have access to the actual data on who dropped out of the panel, I will assume that this is true. However, it is misleading because it apparently includes individuals who turned in blank or incomplete booklets.

---

[5] From the information provided, the cleaning seems to have been somewhat heavy-handed. For example, almost 5% of the fishing trips were dropped because the respondent provided no income data, even though income was not an explicit variable in the model. In such cases of missing data, imputation would have been an alternative to dropping observations.

If one focuses on fishing -- which is the only recreation activity the TER Report considers -- while there were 881 people who identified themselves as having fished over the past year among the members of the MOR panel, *only 303 panel members reported participating in fishing during the 14 months of the MOR survey.*[6] This implies a reporting rate of only 34.4%. Two qualifications should be noted. 173 members dropped out the panel during the course of the survey, some of whom were anglers, which would raise the rate of reporting to some degree; and some respondents who reported fishing trips were probably eliminated during the cleaning of the data. Even with these qualifications, it is remarkable that only 303 of the surviving 976 panel remembers reported participating in fishing during the course of the MOR survey.

I believe that some of this is an artefact of the MOR survey. I noted earlier the failure of the telephone survey to produce results consistent with either the Census Bureau's National Survey on FHWAR or the Montana DFWP Statewide Angling Pressure Survey. It appears that the MOR telephone survey overestimated the number of anglers among adult Montana residents by about 74%. Therefore, instead of 881 anglers among the 1149 members of the MOR panel, there may have been only about 505 anglers in the panel. If the people that withdrew from the panel were evenly distributed among anglers and non-anglers, this would reduce the number of anglers surviving in the panel to about 430. Having 303 individuals report on fishing trips would then constitute a reporting rate of about 70%.

This does *not* mean, however, that there were no significant reporting problems with the MOR panel survey. While there were 303 anglers among the respondents, not all of them seem to have reported participation in fishing consistently throughout the course of the survey. Table 3 presents the data on reporting of fishing participation. July is the peak month for fishing in Montana and, indeed, July 1992 is the peak month for the reporting of fishing trips in the MOR survey. *Thereafter, however, the data show a dramatic decline in the reporting of fishing participation during the course of the survey.* Of 224 individuals who reported participating in fishing during the first wave of the survey (July-August 1992), only 64 reported participating in fishing during the last wave (July-August 1993). It appears that nearly three-quarters of the anglers in the panel at the beginning of the MOR survey had stopped reporting their participation in fishing by the end.

Was the attrition of the MOR panel really as drastic as it seems? I can think of three other possible explanations, but it appears that they can be discounted. Overestimation of the number of anglers in the population as a result of errors in the MOR telephone survey is unlikely to be a factor here because it would affect all waves alike. The elimination of respondents during data cleaning could be a factor but, if anything, I would expect it to work in the opposite direction -- according to the June 13, 1994 Memorandum by Sharon Snow, fewer observations were eliminated during data cleaning for wave 7 than wave 1 because there were fewer incomplete or anomalous responses. Therefore, data cleaning is unlikely to have contributed to the appearance of sample attrition. A third possibility is that there really was a dramatic change of heart among

[6] One of these has a zip code outside Montana.

TABLE 3: REPORTING OF FISHING TRIPS BY PANEL MEMBERS IN MOR SURVEY, 1992-93

		NUMBER OF ANGLERS REPORTING IN EACH WAVE:						
		WAVE 1 (July-Aug)	WAVE 2 (Sept-Oct)	WAVE 3 (Nov-Dec)	WAVE 4 (Jan-Feb)	WAVE 5 (Mar-Apr)	WAVE 6 (May-June)	WAVE 7 (July-Aug)
WAVE OF FIRST APPEARANCE:	WAVE 1	224	89	30	47	47	76	64
	WAVE 2		35	6	7	4	12	10
	WAVE 3			6	2	0	2	2
	WAVE 4				13	2	6	3
	WAVE 5					7	3	0
	WAVE 6						15	2
	WAVE 7							3
	TOTAL	224	124	42	69	60	114	84

adult residents of Montana at the end of 1992 such that the vast majority of anglers suddenly lost interest in fishing in the state. Under this hypothesis, the change in fishing participation is not a survey artefact but a real phenomenon. However, there is no evidence to corroborate this.

That leaves survey attrition and what Bailar called time-in-survey bias as the prime explanations for the huge reduction in the reporting of fishing participation in the MOR panel survey. How likely are these to have occurred? As indicated by the quotation from Sudman and Ferber (1979), it is not uncommon for many participants to drop out of a panel survey after two or three waves. Having three-quarters drop out over 12 months is fairly extreme, but by no means unknown. To counteract this, survey designers adopt a strategy of replenishing a panel by recruiting new members every few waves. In effect, they create a rotating panel. This ensures that, at any one time, not all the panel members have been in the panel for the same length of time and therefore are not all experiencing time- and attrition-related effects to the same degree. This permits one to identify and statistically correct for time-in-survey bias. Replenishing a panel is obviously more expensive than a single recruitment, but it provides superior protection against attrition phenomena.[7]

While attrition is a threat in any panel survey, in the case of the MOR survey there were two additional factors which made it especially likely. The first is that participants were paid their participation fee of $5 per wave regardless of whether they returned a booklet or not, and regardless of how carefully they completed it. While RTI engaged in considerable effort to chase down panel members who failed to turn in a booklet, it seems to have been less concerned about completeness of recording for those who did turn in a booklet. The second factor is that this survey was relatively onerous for respondents. The booklet itself is a lengthy and forbidding document, consisting of 9 separate sections. Even the short version in the TER Report runs to 28 pages. Respondents are asked to record details of every trip for undertaken for the primary purpose of fishing, boating (motor and sail-boating, canoeing, kayaking, rafting, jet skiing, row boating, tubing, windsurfing, water skiing, swimming in lakes, rivers, etc.), tent camping and RV or trailer camping, hiking (day hiking/walking, backpacking, horseback riding, mountain biking), large and small game hunting, duck and geese hunting, recreational trapping, and other activities (picnicking/general park use, wildlife and bird watching, wildlife and landscape photography, rock climbing, horseback riding, sight-seeing). For each individual trip, 20 or more questions had to be answered. For trips with multiple destinations, 54 questions had to be answered. All of this was to be done with pencil and paper in a self-administered survey. It was done not for *some* of the outdoor recreation trips over a period of 14 months, but for *all* of them. From what I know of some people in Montana, answering the questionnaire would itself have been a full-time job. It would be like asking me to account for every cookie that I eat over a period of 14 months. For my part, I would rather spend the time eating than recording! Given the tedious nature of the RTI questionnaire, it would not be surprising if some respondents chose to fill it in selectively, skipping some trips and reporting others incompletely. Indeed, it would be a modern miracle if

---

[7] Recruiting a new panel each wave, as in the Montana DFWP Angling Pressure Surveys, is the ultimate form of panel replenishment. This reduces attrition-related phenomena to the minimum, but requires the most effort for panel recruitment.

this had *not* happened.

In summary, RTI chose to administer an extensive and burdensome questionnaire to a panel of respondents for a relatively lengthy period of time, without replenishing the panel during the course of the survey. Perhaps this was done to save money. Whatever the reason, the results appear to have been fairly calamitous.

The same conclusion arises if one considers the temporal distribution of fishing trips. Table 4 shows the distribution of the fishing trips reported by the 303 anglers in the MOR panel over the survey waves. For comparison, it also shows the distribution of fishing trips for the same time of year reported by 45,384 resident anglers in the 1991 Montana DFWP Angling Pressure Survey (McFarland and Hughes 1994, Table 5).[8] These data are graphed in Figure 1. For both surveys, Figure 1 normalizes the number of visits in July-August to 100, and then traces out the number of visits in each other wave relative to this. The graph covers 14 months, starting and ending with July-August.[9] July is the peak month for fishing by resident anglers in Montana, followed by August. After September, fishing declines considerably as winter approaches. November is the low month for fishing. In January, after the holidays, winter fishing reaches its peak. In April, fishing begins to pick up and, by May, the summer fishing season is under way. It is not surprising, therefore, that the number of fishing trips reported by members of the MOR panel declines in wave 2 (September-October) and reaches its low in wave 3 (November-December). As with the DFWP data, a winter peak occurs in wave 4 (January-February). But then, the two curves begin to diverge. In wave 5 (March-April), trips reported in the MOR survey fall off relative to the DFWP data. This is more pronounced in wave 6 (May-June). In wave 7 (July-August), the non-reporting in the MOR panel is so pronounced that there are actually fewer trips than in wave 6, even though it is now the height of summer. This is a stark depiction of the time-in-survey bias associated with the MOR panel survey.

Not only is the non-reporting in the MOR survey quite extreme, but also it does not occur at random. For example, among respondents those who reported fishing in wave 1, those who said in the telephone survey that they participated frequently in fishing were significantly more likely to continue to report on their fishing through wave 7 than those who identified themselves as participating sometimes or seldom: 34% of the former reported on their fishing in wave 7, compared to 20% of the latter. Similarly, those who took 4 or more trips in wave 1 were significantly more likely to continue reporting through wave 7 than those who took 1 - 3 trips in wave 1; 55% of the former reported on their fishing in wave 7, compared to 23% of the latter. Thus, it was the more active anglers who kept up their reporting of fishing.

The TER researchers were not unaware of the decline in the reporting of fishing trips as the MOR survey progressed. What did they do about it? It appears that they placed their faith

---

[8] The results of the 1993 Survey are not yet available to me.

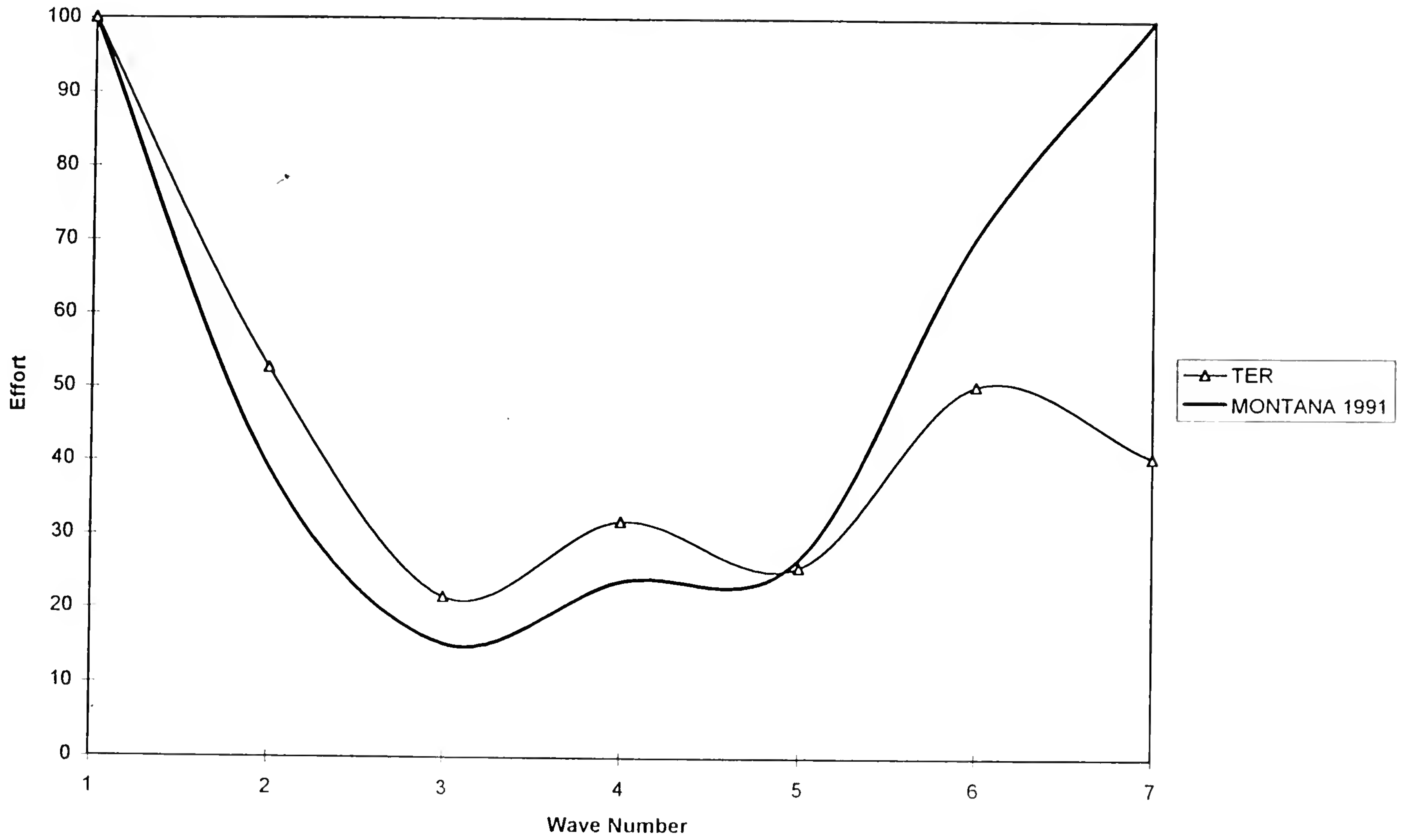
[9] Since the Montana DFWP data cover only 12 months, I use the same data for July-August at the beginning and the end of the graph.

TABLE 4: BREAKDOWN OF RESIDENT FISHING IN MONTANA BY WAVE

	WAVE	RTI - MOR SURVEY (1992-93) (1)	12 - MONTH SHARES	DFWP ANGLING PRESSURE SURVEY (1991-92) (2)	12 - MONTH SHARES
1	July - August	538	35.5%	656,018	36.3%
2	September - October	283	18.7%	256,561	14.2%
3	November - December	116	7.6%	100,106	5.5%
4	January - February	171	11.3%	155,096	8.6%
5	March - April	138	9.1%	175,861	9.7%
6	May - June	271	17.9%	463,803	25.7%
7	July - August	219			
	TOTAL: WAVES 1 - 6	1,517	100.0%	1,807,445	100.0%
	TOTAL: WAVES 1 - 7	1,736			

SOURCE: (1) Fishing trips, calculated from MOR data
(2) Days of fishing from McFarland (1994a), Table 5

Figure 1 Fishing Effort Relative to First Wave



in re-weighting the data. As panel members dropped out of the survey, the weights on the remaining members were correspondingly increased. Evidently the TER researchers hoped that this would offset the shrinkage in panel reporting.

To assess how well this solved the problem, I reviewed the weighted estimates of participation in fishing by adults residents of Montana that are presented in the TER report and compared them with estimates from the National Survey of FHWAR and the Montana DFWP Statewide Angling Pressure Survey. The results are shown in Table 5. Since these other data are developed on a 12- rather than 14-month basis, I consider the weighted estimates of fishing participation from the MOR survey for the period from July 1992 through June 1993.

Using its weights, the TER Report estimates that adult Montana residents took 682,603 fishing trips during this period to the fishing sites covered by its fishing demand model. The model is based on what is known as the random utility maximization (RUM) hypothesis, and I will refer to the sites in the model as the RUM sites. There are 97 of these sites, covering most of western and central Montana. By virtue of their location in the mountains rather than in the eastern plains, they account for the great majority of the fishing in Montana. The Census Bureau estimates that adult Montana residents took 1,557,700 in-state fishing trips in 1991. The Census Bureau does not break its estimate down by region, but this is clearly higher than, and inconsistent with, the estimate in the TER Report.

With respect to trip length, according to the TER Report the average length of fishing trips reported in the MOR survey was 1.45 days per trip. The Census Bureau reports that the average in-state fishing trip by Montana residents lasted 1.2 days. This suggests that the MOR survey systematically over-sampled longer trips.

Combining trip length with number of trips yields an overall estimate of 989,774 days of fishing by adult Montana residents at the RUM sites in the TER Report. The Census Bureau estimates that adult Montana residents engaged in 1,872,000 days of fishing for the state as a whole. The 1991 Montana DFWP Statewide Angling Pressure Survey estimates that Montana residents engaged in 1,807,448 days of fishing in-state. This corresponds very closely with the Census Bureau estimate; it lies well within the Census Bureau's 90% confidence interval. The Angling Pressure Survey breaks down its estimate by individual fishing areas. I have identified the areas that correspond to the RUM sites, and estimate that they account for about 1,591,800 (88%) of the 1,807.448 days of resident fishing in the 1991 Angling Pressure Survey. This suggests that TER's weighted estimate of fishing activity based on the MOR survey underestimates the true amount of fishing at the RUM sites by over a third (989,774 days *vs* 1,591,834 days). If one assumes that the RUM sites account for same proportion of trips by residents anglers to sites in Montana as days of resident fishing, and applies this proportion to the Census Bureau estimate of the number of resident trips statewide, it suggests that there were about 1,371,850 trips by residents to the RUM sites in 1991. Hence, TER's weighted estimate of 682,603 trips annually to the RUM sites is almost exactly half of the true total (49.8%).

To summarize, it appears that there are two biases in the weighted estimates of fishing

TABLE 5: IN-STATE FISHING BY MONTANA RESIDENT ANGLERS

	U.S. CENSUS BUREAU (1991) (12 mos)		RTI - MOR SURVEY (1992-93) (12 mos)	DFWP ANGLING PRESSURE SURVEY (1991-92) (12 mos)
FISHING TRIPS (in thousands)				
ALL SITES	1,557.7			
RUM SITES	1,371.9	**	682.6	
DAYS PER FISHING TRIP				
ALL SITES	1.20			
RUM SITES			1.45	
DAYS OF FISHING (in thousands)				
ALL SITES	1,872.0			1,807.4
RUM SITES			989.8	1,591.8

SOURCE: 1991 National Survey of FHWAR: Montana, Table 3
TER (1995), Table 6-2
McFarland (1994a), Table 6
** Estimated assuming proportion from DFWP Survey

activity based on the MOR survey: an under-reporting of fishing generally, and an oversampling of longer trips. While the biases go in opposite directions, the former predominates and produces a gross undercount of fishing activity at the RUM sites in the TER Report. I conclude that the MOR panel survey did not meet its goal of providing a representative sample of fishing trips by the target population of Montana adult resident anglers. Moreover, the weighting procedures used by TER failed to correct for the errors in survey execution.

There are several reasons why the weights used by TER failed so miserably. First, like everything else in the MOR survey, the weights appear to have been designed for a broad range -- an *extremely* broad range -- of outdoor recreation activities, not just fishing. TER's decision to limit its analysis to fishing must have come occurred relatively late in the analysis phase, after the weights had been determined, because the TER weights are keyed to the returning of survey booklets in general, not to the reporting of fishing. In wave 7, for example, the TER weights reflect the fact that only 666 panel members turned in booklets for that wave compared to 854 for wave 1, rather than the fact that only 84 of them reported fishing in wave 7 compared to 224 in wave 1. This is surely a mistake for a study that now focuses exclusively on fishing.

Second, the failure of the weights in the panel survey illustrates what I said in connection with the MOR telephone survey that weighting is not a panacea and that use of inappropriate weights can be counterproductive. As with the telephone survey, the weights in the MOR panel survey are benchmarked against demographic characteristics of the general population of Montana rather than the population of anglers, and they are designed for measuring population-level statistics rather than parameters of a behavioral model. An example makes the point. Of the 1737 fishing trips reported in the MOR survey, 112 trips (6.5%) come from a single angler, a 63 year old male, one of 9 panel members from Kalispell, who reported in the telephone survey that he fished sometimes but not frequently. In waves 2 and 3, he reported 67 fishing trips, all of them to the Flathead River. As a result of his efforts, 90 of the 1736 fishing trips in the MOR data are to this site, giving it a "market share" of 5.18%. By contrast, the Montana Angling Pressure Survey for 1991 shows that the site accounts for 1.01% of trips by all resident anglers to the RUM sites. Because this individual was retained in the MOR data, the survey overestimates this site's share of fishing trips by over 400%. There is one other person who reported taking over 50 fishing trips -- a 59 year old male, one of 8 panel members from Anaconda, who reported 53 trips, all of them to Georgetown Lake. As a result, Georgetown Lake accounts for 6.6% of the fishing trips in the MOR survey, whereas it accounts for only 2.4% of the trips to the RUM sites in the 1991 Montana Angling Pressure Survey. In this case, because the individual was retained in the MOR data, the survey overestimates the site's share of fishing trips by 175%.

I don't doubt that such anglers exist in the population. But, I do believe that they were weighted inappropriately in the MOR survey. The weights used by TER extrapolate from the MOR panel to the general population of Montana. Consequently, the weights will produce the right number of, say, 63 year old males in Kalispell, or 59 year old males in Anaconda. At the same time, however, they will imply that 1/9 of all the anglers in Kalispell take 112 fishing trips, 63% of them to the Flathead River, or that 1/8 of all the anglers in Anaconda take 53 fishing trips, all to Georgetown Lake. While yielding reasonable population-level demographics, the

weights produce a highly distorted account of angler activity in Montana and its break-down among individual sites and time periods.

Third, no amount of weighting can correct for a grossly undersized survey. The MOR panel was too small to begin with, since it was predicated on a telephone survey that seriously overstated the number of anglers in the Montana population, and it suffered from attrition and growing nonreporting as it progressed. By the last wave, in the summer of 1993, the TER research team was dependent on a sample of just 84 anglers to represent the behavior of perhaps 140,000 anglers statewide. I do not believe that *any* weighting scheme could produce meaningful population-level estimates of fishing activity in those circumstances. To be sure, one can go through the motions of weighting up the 84 anglers. But, the confidence interval associated with the weighted estimate will be enormous -- so large as to render it useless in practice. To the extent the intended application of the MOR survey is to place an economic value on fishing at certain rivers in southwestern Montana, the same conclusion applies with even more force.

3.5 Conclusions

The two-phases of the MOR survey constitute an unusually ambitious and costly attempt to survey outdoor recreation on a fairly large scale, both geographically and temporally. However, because of flaws in the design and execution of the survey, it employed an unrepresentative sample of Montana outdoor recreators and it provides an inaccurate and unreliable account of in-state fishing. The MOR survey simultaneously overestimates the number of adult resident anglers in Montana by 74% and underestimates the number of fishing trips they take at the RUM sites by 50%. It overestimates the length of fishing trips by about 21%. It also provides estimates of the allocation of these fishing trips among the alternative RUM sites that are inaccurate, in some cases by more than 100%.

These errors arose from a combination of circumstances. The telephone survey substantially overstated the extent to which women fish and hunt in Montana, perhaps because of deficiencies in interviewer training and performance. For both male and female respondents, telescoping may have contributed to the overcounting of fishing participation. Because the phone survey greatly exaggerated the number of anglers, the MOR panel actually contained far fewer anglers than the TER research team had anticipated. I estimate that it contained only about 505 anglers -- much too small a sample for the ambitious goal of measuring the allocation of fishing effort among 100 different locations over one winter fishing season and two summer seasons. This was compounded by tremendous panel attrition and nonreporting as the survey proceeded. The main reason for the attrition and nonreporting was the tedious and burdensome nature of the questionnaire, combined with the lack of incentive for complete reporting. The research team failed to offset this by replenishing the panel during the course of the survey. The attrition and nonreporting did not affect panel members at random; it was the more active anglers who kept up their reporting, leading to a form of avidity bias in the sampling. By the summer of 1993, the MOR survey was dependent on 84 relatively avid anglers to represent the fishing behavior of about 140,000 anglers statewide. The TER research team relied on weighting to correct for all

the sampling inadequacies. In this case, the faith in weighting was misplaced. Indeed, the weighting seems to have compounded the problem because since it was keyed to the general population of Montana rather than the target population of anglers. Consequently, the weighting actually increased the error in the MOR data.

Although the survey problems arose from somewhat subtle causes, their aggregate effect is gross and unmistakable. To detect this, one merely has to compare the results of the MOR survey with other existing data on fishing in Montana such as the Census Bureau's National Survey of FHWAR. It doesn't take a rocket scientist to do this.

Why does the TER Report not mention these obvious problems with the MOR survey? Is the reason complacency -- a belief that, since this was the most lavish economic survey of outdoor recreation up to now, its results must be uniformly excellent? Is the reason strategic -- if the survey data are flawed, this destroys the credibility of the economic model estimated from the data? I suspect a bit of both. Whatever the reason, however, the failure to address the flaws in the MOR data is a serious indictment of the TER research team.

Moreover, it throws the rest of their study in jeopardy. The central position of the TER Report is that one can make inferences from the sample in the MOR survey to the whole Montana population. The fact that the inferences produce results so much at variance with other information on the Montana population leads me to doubt this claim.

4. The Variables in the RUM Model

4.1 Introduction

This section reviews the variables used in the RUM model that appears in the TER Report. The dependent variables in the model are measures of participation in fishing at the RUM sites; these are discussed in Section 4.2. For the purpose of placing an economic value on fishing, the key explanatory variable is the "price" variable -- the cost that anglers associate with going fishing. This is discussed in Section 4.3. The other key explanatory variable needed to assess the loss of consumer surplus from pollution in the Upper Clark Fork River Basin is one or more measures of how the pollution affects angler behavior and welfare -- i.e, measures of the aspects of fishing that would affected by the pollution, such as the quality of the fishing experience. Section 4.4 discusses this variable. The effects of measurement error in this variable are examined in Section 4.5. Other site characteristics are considered in Section 4.6

4.2 The Dependent Variables

The dependent variables in the RUM model are various aspects of participation in fishing, including:

(1) how often the individual participates in fishing (number and frequency of trips)

(2) the length of the fishing trip, and

(3) the type of trip (river *vs* lake) and the specific site destination.

Because of the flaws in the MOR panel survey, as discussed above, all of these variables are measured with error.

Compared to the Census Bureau's data, there is only half the correct number of trips by resident anglers to the RUM sites. This biases the estimation of participation frequency and renders it unreliable. Similarly, more longer trips are reported in the MOR survey than is indicated in the Census Bureau's data, which biases the estimation of trip length and renders that part of the TER model unreliable. The breakdown of individual trips among sites in the MOR is also suspect. I already mentioned the effects of two outliers, the angler in Kalispell who reported 112 trips, 90 of them to the Flathead River, and the angler in Anaconda who reported 53 trips, all to Georgetown Lake. Including these cases biases these sites' shares of total trips in the MOR data, and causes them to diverge greatly from the shares in the Statewide Angling Pressure Survey for 1991. This particular error can readily be corrected by omitting the two individuals from the data used to estimate the model, but there are other errors in the site shares that are less readily corrected. For example, the breakdown of trips in the MOR data between sites which are rivers versus sites which are lakes differs significantly from what the Angling

Pressure Survey shows for the RUM sites. As shown in Table 6, the MOR survey overestimates the share of trips to river sites in the summer, and underestimates it in the winter; in both cases, the difference is statistically significant. Such errors bias the estimation of site choice and render this part of the TER model unreliable.

Another worrisome feature of the site shares in the MOR data is the tiny number of anglers from whom they are derived. As shown in Table 7, with one exception (an angler with a Northern California zipcode), all the anglers in the MOR survey come from one of seven regions in the state. In four of the regions, there were 40 or fewer anglers reporting on their fishing. These are extremely inadequate samples if one is serious about modelling the choices that anglers in these regions make among almost 100 destinations in an area about two-thirds of the state. Of the members of the MOR panel, 283 individuals reported trips during the summer fishing season (defined by Montana DFWP as May 1 - September 30), and 141 reported trips during the winter fishing season (October 1 - April 30). Twenty of the anglers in the MOR panel (6.6%) reported fishing in winter but not summer. In the Kalispell region, 6 out of 35 anglers (17.1%) reported fishing in winter but not summer. My hunch is that the true proportion of Montana residents who fish in the winter but not the summer is much smaller than these figures indicate. If this is so, the error in the MOR data would throw the seasonal element in the fishing participation model out of kilter. Table 7 also shows that 121 of the 283 anglers in the MOR survey who reported fishing in the summer (42.8%) also fished during the winter. I suspect that the true proportion is lower and that the MOR data are an artefact resulting from over-sampling of avid anglers among those who reported.

4.3 The Cost Variable

The cost that anglers associate with going fishing is crucial for placing an economic value on their fishing activity. It was the identification of this cost that led Hotelling (1949) to his discovery of the basic method for valuing recreation and similar nonmarket activities. Indeed, because of its central role, the method is commonly known as the "travel cost method" (TCM).

Just how one measures the cost in practice is something that economists have considered since the first applications of the TCM by Trice and Wood (1958) and Clawson (1959). Clawson expressed the situation thus: " The costs of outdoor recreation experiences are partly subjective, partly material. Some cash will have to be spent for travel, food, lodging if more than one day is involved, and for other purposes. The use of equipment such as the automobile is usually included also. ... The time and energy spent in travel will be a cost, which may or may not be offset by pleasures from the actual travel experience." The various categories of expenditure that Clawson mentioned -- motor vehicle or other transportation cost, on-site recreation costs (entrance fees, tackle and rental equipment, etc), costs of food and accommodation, and time costs -- have formed the basis for cost estimation in the subsequent TCM literature.

There is a fair amount of consensus in the literature about how these costs should be estimated. One approach is to survey recreationists and ask what were their expenditures for

TABLE 6: DISTRIBUTION OF TRIPS BY RESIDENT ANGLERS AMONG RIVER AND LAKE RUM SITES

		% RIVER	% LAKE
SUMMER	RTI - MOR	56.4%	43.6%
	MONTANA DFWP	50.5%	49.5%
WINTER	RTI - MOR	43.7%	56.3%
	MONTANA DFWP	55.7%	44.3%

TABLE 7: NUMBER OF ANGLERS REPORTING TRIPS IN MOR SURVEY, BY REGION

REGION	ZIP CODES	NUMBER OF INDIVIDUALS REPORTING		
		WINTER TRIPS	SUMMER TRIPS	EITHER
BILLINGS AREA	590xx, 591xx	15	32	35
GREAT FALLS AREA	594xx	18	39	40
NORTH CENTRAL AREA	595xx	4	5	6
HELENA AREA	596xx	26	52	54
SOUTH WEST AREA*	597xx	33	63	68
MISSOULA AREA	598xx	24	62	64
KALISPELL AREA	599xx	21	29	35
OUT OF STATE			1	1
TOTAL		141	283	303

* Includes Bozeman, Butte and Anaconda

various items. The other approach is to impute costs based on information from some external source, for example Automobile Association data on the costs of owning and operating a vehicle, or what the Internal Revenue Service or other government agencies allow individuals for the use of their automobile. The main issue with respect to automobile travel is whether or not one counts all of the costs. According to the American Automobile Manufacturers Association, as reported in the *Statistical Abstract of the United States 1994*, the average cost of owning and operating an automobile in the US in 1992 was 45.8 cents per mile, broken down into 9.2 cents per mile for operating expenses (gas, oil, tires, and maintenance), 27.8 cents per mile for depreciation, and 17.6 cents per mile for other fixed costs (insurance, license and registration, and finance charge). In that year, the IRS allowed a standard mileage rate of 28 cents per mile, while the State of Montana allowed its employees a mileage rate of 27 cents per mile; these mileage allowances essentially cover operating costs plus depreciation. A majority of the studies in the TCM literature that impute a transportation cost use something like the government mileage rate.[10] However, some studies exclude depreciation on the grounds that this is a fixed expense which should not influence people's choice of destination for recreation. Of course, whether or not it *does* influence their choices is an empirical question that could be resolved, for example, by questioning them.[11]

The MOR panel survey did request respondents to report what they saw as transportation and other costs. However, the TER research team chose not to use the survey responses. Instead, it imputed a cost based on respondents' reported miles per gallon combined with an external estimate of the cost of gasoline per gallon and the cost of oil, tires, and maintenance. The gasoline cost averages about 6 cents per mile, and the other costs are imputed at 5 cents per mile, for a total transportation cost in the TER Report averaging about 11 cents per mile. I assume that this is lower than what the survey responses show.

With regard to monetary expenditures for non-transportation items, the TER research team excludes these entirely. This is a relatively extreme position. Most other TCM researchers include some of the on-site recreation costs or food and lodging costs, especially for longer trips. Indeed, I do not know of any other study in the literature which has modelled trip length as a endogenous choice variable while excluding from the analysis costs such as food or lodging that might be expected to vary with trip length.

With regard to time cost of recreation, the TER research team include travel time, valued at one third of the wage rate, but exclude time spent on-site. It is not uncommon in the literature to ignore on-site time when dealing with short trips. When dealing with long trips and the choice of trip length, however, this makes less sense because the time commitment is likely to be a

---

[10] For example, this is true of most of the studies listed in Desvousges et al. (1986), Table 2-1.

[11] People may decide to buy a particular type of vehicle for their second car precisely because it will be more useful for the type of outdoor recreation they like. If so, the fixed costs of vehicle ownership are directly attributable to their recreation activity. The fact that respondents in the MOR survey usually take a truck to go fishing (see Table 6.3) is certainly consistent with the argument that fixed costs should be counted.

significant component of the cost associated with taking longer trips.

There is disagreement in the literature with regard to the valuation of travel time. Some researchers impute a value of time whereas others estimate it from the data. Those who impute a value of time typically make it some specified fraction of the wage rate. Cesario (1976) suggested valuing travel time at one third of the wage rate, based on findings in the literature dealing the choice of transportation mode when commuting to work. Others have suggested using a higher fraction. The TER Report seems to imply that one third of the wage rate is some sort of upper bound on the opportunity cost of recreation travel time: "because recreation travel is different than commuting to work, it is reasonable that the opportunity cost of recreation travel time is lower than that associated with commuting." This is not a position that Dr. Desvousges has always espoused. Earlier in his career when he was conducting research aimed at publication in academic journals, rather than intended for use in litigation, he took the position that the effect of time constraints on choices by recreationists is a complex matter which will vary with circumstances and that "simple approximations to relate the opportunity cost of time to the wage rate will not be able to accommodate all applications" (Smith, Desvousges and McGivney, 1983). However, given that one needs to use some such approximation, while Cesario's proposal to value time at one third of the wage rate sometimes fits the data well, at other times one gets a better fit by assuming that travel time is valued at 100% of the wage rate: "Cesario's proposal is not unambiguously superior to using the wage rate as the opportunity cost. Indeed, until better information on the nature of time constraints facing individuals in their recreation decisions is available, the wage rate provides an equally plausible approximation for the opportunity cost of travel time" (*op. cit*).

I believe that Dr. Desvousges' earlier position is considerably more reasonable than the one he now adopts. In my own research I have shown that, in the face of constraints on one's time, the opportunity cost of time for recreation can substantially exceed the wage rate and need not be bounded by the opportunity cost of time spent commuting to work. Therefore I certainly cannot regard valuing travel time at one third of the wage rate as a conservative assumption.

In summary, the TER research team have chosen ways to measure the travel cost variable that are likely to bias it downwards to some degree. Their costing of automobile travel at 11 cents per mile, while within the range encompassed by the existing literature, is not the majority position. Their exclusion of all other monetary costs is generally inconsistent with the normal practice in the literature for the type of recreation trips involved. Finally, as indicated above, their valuation of the opportunity cost of travel time at one third of the wage rate is by no means a conservative assumption.

Without having access to the full data from the panel survey, I am not in a position to verify the computation of the travel cost variable in the RUM model or to assess the likely magnitude of its bias. Based on the information available to me at this time, my rough guess would be that the costs are underestimated by a factor of two for short trips, and considerably more for long trips.

Why does this matter? Is the measurement of the travel cost variable merely an arcane academic nicety, or does it have some practical implications?

Obviously, correct measurement of variables is important for the accuracy and reliability of a statistical model. However, it is especially important in the case of the travel cost variable because this serves to scale all economic values derived from the recreation demand model. Hence, scaling errors in the measurement of travel cost translate directly into scaling errors in the measurement of economic value. The result can be demonstrated mathematically, but it also has an intuitive explanation. Suppose the travel costs are underestimated by a common scale factor. As a specific example, suppose they are underestimated by a factor of two -- for each person in the sample, his travel cost for every site is mistakenly halved. His true cost is, say, $6.00 per trip for site A and $8.40 per trip for site B, but in the data from which the model is estimated these costs are erroneously recorded as $3.00 and $4.20, respectively. From the point of view of the computer estimating the model, it is as though there has been a change of monetary units from units of $1 to "new" units of $2. It sees the prices of the sites in terms of the new units as 3 and 4.2, respectively. Consequently, when the coefficients of the model are estimated, they are to be interpreted in terms of the "new" units. So are estimates of consumer's surplus derived from the fitted model. Hence, if costs are underestimated by 50%, so are dollar estimates of consumer's surplus. Thus, to the extent that travel costs are biased downwards by a common factor in the TER model, the estimates of lost consumer's surplus in the Upper Clark Fork River Basin will be biased downwards by same proportion.

4.4 Site Characteristics

In addition to travel cost, the RUM model uses several other variables to measure fishing site characteristics -- aspects of sites that might give utility (or disutility) to anglers and influence their choice of destination. Section C.2 in the TER Report gives a long list of site characteristics that were considered for the RUM model. The great majority of them seem to have vanished without trace -- they are not used in the model as reported, nor is there any discussion of the reasons why they were not used or how their inclusion would affect the coefficients of the variables that are included.

The model presented in the TER Report employs seven site characteristic variables in addition to travel cost. Four are common to choices among both river and lake sites; three vary by choice model. The common characteristics are a dummy variable for whether the site is identified as a major fishing site in *The Angler's Guide to Montana*, the log of the area of the site as measured in USGS map blocks, the number of campgrounds at the site per USGS block, and the number of State Recreation Access (SRA) areas at the site per USGS block. While acreage of area may be a good measure of size for land-based activities such as hiking and camping, it seems less appropriate for fishing at water bodies. For fishing, the amount of shoreline may be a better measure of size than simple area. Further, while crowding may be an attribute that is important to anglers, there may be other variables that capture it better than area. With regard to using the number of campgrounds or SRA areas as measures of site quality, there

could be some endogeneity: the establishment of campgrounds at a site, for example, could reflect the fact that this is an attractive site -- it is a consequence rather than a cause of what one seeks to measure. Similar with the dummy variable for whether a guide book rates this as a major site. This tells you that the site is popular without saying why. It may be highly correlated with the dependent variable not because it explains visitation in some legitimate manner but rather because it is essentially another measure of visitation.

In the case of the choice model for river fishing, the other three site characteristics are the number of restricted species at the site, the biomass of trout measured in pounds per 1,000 feet of river, and a dummy variable for the aesthetics of fishing at the site. In the choice model for lake fishing, the variables are a dummy variable for lakes with good access, a dummy variable for lakes with abundant fish, and a dummy variable for the aesthetics of fishing at the lake site. Of these, the biomass variable is the crucial one, given the intended application of the model. The variable has two roles. First, it contributes to the model's explanatory power with respect to the allocation of fishing trips -- it helps explain why an angler chooses one site over another when they both have the same travel cost. Second, it is the key link for computing economic damages from the release of hazardous substances in the Upper Clark Fork River Basin. The damages from these releases are incorporated into the RUM model as changes in the value of the biomass variable. If the change in the biomass variable does not adequately capture their effect on anglers, this will bias the estimate of lost consumer's surplus -- if it understates the impact on anglers this biases the damage downwards, and conversely if it overstates the impact on anglers. Similarly, any measurement error or statistical procedure that biases the coefficient estimate for the biomass variable will correspondingly bias the estimate of lost consumer's surplus.

Thus, the challenge for the researcher is to identify an indicator of the quality of fishing at a site that can be measured with reasonable accuracy, captures the aspects of the site that motivate angler behavior, and can reasonably be used to represent the effects of releases of hazardous substances in the Upper Clark Fork River Basin.

For river sites, the TER research team selected for this purpose a variable from the Montana Rivers Information System (MRIS), a computerized data base maintained by the Montana DFWP, that purports to measure the pounds of trout biomass per 1000 ft. of stream length. This information is provided for individual rivers, streams and creeks, or designated sections thereof. In some cases. MRIS gives no data for a particular area; I understand that, in those cases, the TER research team used some sort of regression equation to impute missing values (as far as I can tell, this is not discussed in the TER Report). In many cases, several different streams or sections of streams are contained within a single RUM site. In those cases, the TER research team took a weighted average of the biomass figures for the individual areas, using stream length for weights. This seems less appropriate than weighting on the basis of relative visitation by anglers, or on the basis of some variable that captures angler's expectations

regarding the site.[12]

The MRIS data base does not cover lakes. Therefore, in place of the MRIS measure of biomass, in the lake choice model the TER research team used a dummy variable taking the value of one for lakes with "abundant" fish and zero otherwise. This was based on information from the Montana Interagency Stream & Lake Database. The Report gives no further information about how biomass is characterized in that database or how the research team converted it into a binary-valued variable. Using a binary-valued variable is a coarse way to capture attractiveness to an angler of fishing at a lake site. If one is serious about modelling anglers' choices of lake fishing, as the TER Report claims to be, it is hard to believe that one would be satisfied with such a meager characterization of so important a variable. On its face, therefore, this appears to be an inadequate treatment of fishing quality in the modelling of choices among lakes relative to what is done for choices among river sites.

With rivers, however, although the TER research team might have thought they had found a good indicator of fishing quality, they eventually learned that this was not so. They were informed in 1994 that the version of the MRIS database which they had used (Version 2, issued in 1989), and especially its biomass data, was inaccurate and highly unreliable. Of the records in that version with biomass data, 62% had data collected before 1980, and 70% had a data with a low reliability rating. The information on the age and reliability of the data had not been available in the old version of MRIS, but it was included in a new version released in February 1994 (version 3). In addition, the new version incorporated more recent biomass data where this had become available, and it now provided data by smaller and better defined river segments.

This must have been a devastating blow to the TER research team.[13] At least, it would have been for *me* if I had been in their position. The key variable on which one is counting to drive the site choice model suddenly turns out to be grossly unreliable and out of date. The TER Report states on page 30 that its objective was to collect site data relevant to the time period of the panel survey (July 1992 - August 1993). Site attributes measured in the 1970's hardly seem to meet this requirement.

I believe that this problem was to some extent one of the TER researchers' own making. Most researchers in their position would have insisted on finding a site quality variable that was measured contemporaneously with the recreation data. In my own work, I have devoted enormous effort to obtaining site quality variables that are both meaningful and pertinent. In my study of beach recreation in Boston, I arranged to have water samples collected from beaches while the survey was in the field, and I also consulted contemporaneous Department of Health records of

[12] To the extent that trout is not a target species at some RUM sites, trout biomass would be the wrong type of biomass to use as a quality indicator.

[13] But it should not have been unexpected. The 1989 version of MRIS which they used bore the disclaimer that "any data retrieval from MRIS is not intended as a final statement on river related resources and should not be used as a substitute for on-site field surveys needed for resource evaluation."

water quality at the sites. In my Southcentral Alaska study, I used contemporary reports of fishing conditions and catch to track the quality variable during the course of the survey. Using a variable measured at some unknown time in the past is asking for trouble.

How did the TER research team react to the news that the variable they were depending on as their key measure of site quality was so inaccurate and out-dated? Did they drop this variable? Did they consult the new MRIS database in order to make use of the new information on biomass? As far as I can tell, the answer to both questions is "no." Neither their Report nor any of their internal documents available to me suggests that they either inspected the new database or gave any consideration to modifying their biomass variable. Instead, they embarked on a Monte Carlo simulation study of some hypothetical level of measurement error which, they purported, placed an upper bound on the degree of bias in coefficient estimates and estimates of consumer's surplus. In my view, this was a serious error in judgment. Using a variable known to be so problematic without checking to see what light the new database might shed on these problems is irresponsible.

I have examined both the old and the new versions of MRIS and compared the trout biomass measurement used in the TER Report against the data in the new version of MRIS. The results are shown in Table 8. Based on my analysis, I have reached the following conclusions:

(1) Although there are some new (post-1989) data on trout biomass in version 3 of MRIS, most of the data are not new, as can be seen from Table 8, and were already used in the 1989 version.

(2) Even in the new version of MRIS, most of the biomass data are extremely old. Of the sites with data, 9 sites have no data after 1976, and another 13 have no data after 1984. At many other sites, even though there are some later data, the 1976 data constitute a significant fraction of the available information.

(3) In most cases, the data are very sparse. Often, there is a single data point for most of a river -- sometimes for an entire river -- and this data is imputed to every other segment of the river in order to fill out the database. On the main stem of the Kootenai River, for example, there is a single piece of data on rainbow trout biomass, dated 3/1/81, and this same value is listed with monotonous regularity for all 15 reaches of the river.[14] Similarly with 12 of the 15 other RUM sites listed on the first page of Table 8 -- there are only one or two pieces of data on biomass and these are imputed to all the other parts of the site. In other cases, the biomass is simply listed as zero. In these cases, zero is not a measure of biomass but, rather, an indication of "no data." This happens with 14 of the RUM sites.

(4) In most of RUM sites where the data are sparse, they are also of low reliability. MRIS uses a scale of 1 - 9 to rate the quality of the biomass data, where a rating of 1 - 3 denotes data based on judgment (i.e., a guess); a rating of 4 - 6 denotes data based on limited measurements; and

[14] I skip these repetitions in Table 8.

TABLE 8: ESTIMATES OF BIOMASS (POUNDS PER 1,000 FOOT OF RIVER)

RUM SITE	BIOMASS IN TER REPORT	TROUT BIOMASS FROM NEW MRIS DATABASE (VERSION 3, 1994)
Kootenai River	86	10.8 (1986); 81.6 (1981)
Yaak River	9	NO DATA
Upper Flathead River	71	77 (1985)
Stillwater & Whitefish Rivers	13	7.3 (1976)
Flathead River	157	154 (1985);
Middle Fork Flathead River	78	77 (1985)
St. Mary River etc	24	NO DATA
Lower Clark Fork	57	NO DATA
St. Regis River	47	87.8 (1976)
Flathead River	70	NO DATA
Middle Clark Fork River	57	440 (1976); 26.4, 4.4, 2.2 (1985); 5.3, 1.5, 39.6, 132, 121, 143,10.6, 9.7, 114.4 (nd) 52.8, 2.4, 39.6, 5.1, 1315.6, 4.6, 1227.6 (nd); 10.8 (1986)
Swan River	90	NO DATA
South Fork Flathead River	9	NO DATA
Teton River	16	5.7, 22 (1976)
Sun River	29	5.7 (1976)
Dearborn River	19	22, 2.2 (1976)
Lower Missouri River	235	82.4 (1969);15.4, 19.1 (1980); 502, 93.9, (1983);602, 98.8 (1989); 132.4 (1976); 355.7, 44.4, 320.1, 40 (1983); 46 (1989)
Smith River	63	22, 110.7 (1976)
Belt Creek	14	22, (1976)
Lower Bitterroot River	123	132.4 (1976); 63.4, 25.5 (1984); 29 (1975)
Rock Creek	167	107.8 (1979); 13.2, 39.6 (nd)
Blackfoot River	70	103.4, 11 (1982); 26.4, 66 (1976) 4.2, 6.2 (1991); 9.2, 7.9 (1989); 14.7, 3.5 (1990); 20.5 (1982); 4.4 (1985)

TABLE 8 continued

RUM SITE	BIOMASS IN TER REPORT	TROUT BIOMASS FROM NEW MRIS DATABASE (VERSION 3, 1994)
Clark Fork River (Garrison-M	30	24.2, 2.2, 30.8 (1985); 32.8 (1989); 18.7 (1990); 25.7 (1991); 4.4, 8.8 (1985) 8.6, 8.1, 1.1 (1989); 5.5, 4, 25.5 (1990); 43.6 (1989); 22.7, 60.7 (1990); 51.9 (1991) 20.5 (1982); 4.4, 37.8 (1985); 52.8 (1989); 21.3, 81.4 (1990); 57, 77, 34.1 (1991)
Clark Fork River (Warm Spri	94	27.3 (1984); 67.1, 33.9 (1989); 36.3, 43.8 (1990); 37.2 (1991); 68, 75.5 (1989); 47.3, 78.1, 70.6 (1990) 68.4 (1991); 78.8 (1992); 72.6 58.5 (1989); 36.5, 73.7, 53.7 (1990); 84.3 (1991) 49.9 (1992); 216.7 (1984); 149.8, 57.4, 106.3, 24.6, 152 (1989); 207, 326.7, 134 (1990); 58.3, 3.1, 137.9, 51.7 (1991); 207, 120.6 (1992)
Flint Creek	67	72.6 (1976); 50.2 (1983)
Warm Springs Creek	30	113.1, 152.2, 187.7 (1989); 60.9 (1990); 48.8, 45.5, 38.9 (1991)
Little Blackfoot River	50	84.7 (1983); 14.1, 30.6, 51, 35.6 (1990); 23.8, 54.1, 85.4, 48 (1991)
Tenmile & Prickly Pear Cree	50	72.6, 2.9 (1976)
Boulder River	23	70 (1974); 16, 41, (1976)
Judith River	6	NO DATA
Musselshell River	60	43.8 (1976); 66, 36 (1985); 42 (1987); 22 (1988); 64.9 (1985)
Upper Bitterroot River	73	63.4, 25.5 (1984); 33 (1985); 4, 8 (1992); 43.8; 52.8, 13.2 (nd)
Upper Big Hole River	56	132.4 (1976), 87.8 (nd); 3.7, 21.1 (1983);64 (1980)
Lower Big Hole River	137	64 (1983), 167.6 (1984), 132.4 (1976); 130, 60 (1986); 10 (1981); 8.8 (1980)
Beaverhead River	260	109.6, 220 (1976); 444.6,424.4 (1984)
Ruby River	53	66.4 (1982); 25.7, 13.9 (1976); 83.2, 24, 15, 13(nd); 5.9 (1976) 8, 13.9 (1975); 12 (1970); 3.1 (1976)
Jefferson River	101	10.8 (1986); 61.8 (1985); 114 (nd)
Madison River	316	42, 107, 24, 75 (1989); 48, 54 (1992); 23, 44 (1994) 41, 259 (1968); 87, 267 (1969); 69, 275 (1970); 87, 234 (1971) 56, 298 (1972); 62, 243 (1973); 122, 248 (1974); 111, 308 (1975) 91, 144 (1976); 129, 202 (1977); 189, 286 (1978); 286, 291 (1979); 356, 266 (1980); 358 (1981); 273 (1982); 217, 311 (1983); 117, 229 (1986); 127, 219 (1987); 184, 365 (1988); 167, 467 (1989); 88, 360 (1990)

TABLE 8 continued

RUM SITE	BIOMASS IN TER REPORT	TROUT BIOMASS FROM NEW MRIS DATABASE (VERSION 3, 1994)
Missouri River	243	136.4 (1979); 45.8 (1982); 396 (1976)
Upper Madison River	364	93.7, 273.9 (1984); 11, 137 (1967); 25, 127 (1968); 33, 118 (1969); 54, 194 (1970); 113, 29, 20, 60, 251 (1971); 52, 93, 6, 9, 129, 14, 40, 227 (1972); 24, 130, 156, 67, 167 (1973); 89, 203, 43, 214 (1974); 121, (1975); 169, 288 (1976); 95, 262 (1977); 167, 262 (1978); 75, 336 (1979); 80 (1980); 127, 303 (1981); 127, 303 (1982); 148, 285 (1983); 109, 332 (1984); 140, 206 (1986); 110, 176 (1987); 110, 187 (1988); 142, 214 (1989); 81, 306 (1990); 101, 245 (1991); 105, 269 (1992); 41, 281 (1993); 71, 89 (1982); 217, 165 (1983); 199, 53 (1975); 117, 61 (1976); 348, 141 (1977); 257, 178 (1978); 235, 129 (1979); 209, 89 (1980); 1959, 112 (1981); 234, 205 (1982); 191, 126 (1983); 213, 145 (1984); 316, 186 (1985); 227, 161 (1987); 261, 600 (1994); 233, 130 (1977); 304, 200 (1978); 200, 164 (1979); 175, 91 (1980); 742, 139 (1981); 385, 275 (1982); 287, 241 (1983) 349, 248 (1984); 307, 213 (1985); 233 (1986); 255, 211 (1987); 414, 262 (1988); 338, 392 (1989); 275, 154 (1990); 122, 142 (1991); 139, 145 (1992); 92, 211 (1993); 206.1, 143.7 (1984); 129.8, 56.5, 112.9, 336 (1976); 108 (1970); 72.6 (1976)
East & Lower Gallatin Rivers	100	53.5, 62.9 (1976); 81.6 34.5 (1984); 24.9, 419 (1980); 82.3 (1984) 89.8, 175.8 (1976); 197 (1977)
Upper Gallatin River	167	256 (1984); 43.8 (1976); 536 (1984);18 (1975); 21.8 (1976); 7 (1979)
Upper Yellowstone River	119	30.6, 73.5, 26 (1986); 20.9, 77, 12.3 (nd)
Middle Yellowstone River	110	83.2 (1983); 38.5, 39, 65 (1984); 34, 43, 2.3 (1986); 25.1, 58.5, 14.7 (nd) 167.9, 59, 5.9 (nd)
Boulder River	67	52.6 (1983)
Stillwater River	70	99.2 (1982); 7.5 (1986); 4.4, 31.5 (1986);42 (1989); 73 (1976); 27, 43.6 (1981) 36 (1985); 12.5 (1986); 41.7 (1989); 43.8 (1976); 16, 65 (1989); 62.5 (1986)
Red Rock River	69	NO DATA
Grasshopper & Prairie Creek	15	6 (1980); 39 (1992), 33 (1986)
Little Bitterroot River	22	NO DATA
Lower Yellowstone River	155	175.8 (1976); 39, 65 (1984); 34, 43 (1986); 83.2 (1983); 38.5, 39, 65 (1984) 34, 43, 2.3 (1986); 45.8, 26.2 (1982); 175.8 (1976)
Silver Bow Creek	0	NO DATA

TABLE 8 continued

RUM SITE	BIOMASS IN TER REPORT	TROUT BIOMASS FROM NEW MRIS DATABASE (VERSION 3, 1994)
Missouri River (Fort Benton)	111	NO DATA
Bighorn River	362	106 (1984); 372.5 (1985);1818 (1987);
Clarks Fork Yellowstone Riv	47	NO DATA
Yellowstone River (Billings)	40	29, 17, 63, 30, 41 (1988);
Bearpaw Mountaiins	28	29.9, 21.3, 6.2, 29.3, 43.8 (1980)
Missouri River (Five Lakes)	590	363.4, 216.5 (1983); 264 (1976)
Red Lodge & Willow Creeks	6	NO DATA
Ashley Creek	3	NO DATA

a rating of 6 - 9 denotes data based on extensive measurements. At the sites with sparse biomass data, the quality rating is generally 3 or lower.

(5) Even where the biomass data is sparse, it can also be highly variable. The biomass value in one part of a river can be substantially different from that in another; or, the biomass value in the tributaries can be substantially different from that in the mainstem. At the Kootenai River RUM site, for example, there is a single biomass value of 10.8 in the tributaries and a single value of 81.6 in the mainstem. Similarly, with the Teton River, the Dearborn River, the Smith River, the Lower Bitterroot River, and many other RUM sites. In this situation, using a weighted average of the two biomass values (computed to two decimal places) is virtually meaningless. Indeed, using any single measure of biomass without trying to represent the heterogeneity of conditions at the site does not adequately capture the quality of the fishing that anglers experience. The aggregation of disparate areas into a single RUM site would still be a severe problem even if the data were not of such low quality and so many years out of date.

(6) At a few RUM sites, instead of sparse data on biomass, there is extensive coverage both spatially and temporally. This is especially true of the Clark Fork and Madison Rivers. However, these rivers illustrate the extreme variability that one finds with biomass across repeated measures. Figure 2 provides an illustration -- it is a page taken at random from the many pages of data on the Madison River in the new version of MRIS. In this case, the units are pounds per mile of river rather than per 1,000 ft. It shows, not surprisingly, that biomass values can be very different for different species of trout -- in this case, rainbow trout versus brown trout. Even for a given species of trout at a given location, the biomass values vary greatly not only from year to year but also across repeated measurements in the same year. With brown trout, for example, there are two separate readings in 1971, one using the mark-recapture method on 8/17/71 and the other using a log-likelihood estimate on 9/22/71; the first generates a biomass estimate of 599, while the second generates an estimate of 1326.6, more than twice as large. In 1972, there are two separate estimates based on the mark-recapture method, dated 4/26/72 and 9/5/72, and a third estimate using the log-likelihood method on 9/29/72. These generate estimates, respectively, of 492, 682, and 1280, getting on for a three-fold variation. The various estimates of rainbow trout biomass in 1971 and 1972 show a similar variation: 154, 319, 273, 47 and 212. At these sites, the variation in biomass values across measurement techniques and species of trout, and over time (both different seasons within the year and from one year to another) is compounded by immense spatial aggregation.

(7) In addition to the problems with sparse, unreliable, and outdated data, there are several sites where the TER biomass values appear inconsistent with the data in the new version of MRIS. At the Missouri River (Five Lakes) RUM site, the TER biomass value of 590 is about twice what the MRIS data suggest; at the St. Regis River RUM site, it is about half what the MRIS data suggest. Other sites where there are serious inconsistencies include the Sun River, Belt Creek, and Warm Springs Creek. In addition, there are the sites where the new version of MRIS offers no data on biomass, yet there are biomass values in the TER Report.

(8) Even if one had a reliable and contemporaneous measure of trout biomass at a site, and even

FIGURE 2: SAMPLE SCREEN FROM MRIS VERSION 3

```
                    Montana Department of Fish, Wildlife & Parks       10/14/1995
                        Montana Rivers Information System

MADISON R
 ODELL CR - BLAINE SPRINGS CR

              ááááááááááááááPopulation Estimatesáááááááááááááá  (continued)
                              Data per mile
 Date        Estimation Method          Sampling Gear
                                  Biomass  Avg Len  Avg Wt  Min - Max    Per
  Species               Total      (lb.)    (in.)    (lb.)    (in.)      Comp  DQR
   Coll. Code  Collector
ááááááááááááááááááááááááááááááááááááááááááááááááááááááááááááááááááááááááááááááá
  Brown Trout       1719þ172.30  1024.0    14.90     1.70   5.00-25.90    76   (5)
   370012      VINCENT,D
 08/17/1971 Peterson mark-recapture         Boat shocking - mobile anode
  Brown Trout       1408þ  0.00   599.0    13.70     1.30   5.00-20.40    79   (5)
   371016      VINCENT
 08/20/1971
  Rainbow Trout      230þ  0.00   154.0    11.10     0.65   6.20-17.30    13   (5)
   371016      VINCENT
                     167þ  0.00   108.0    10.10     0.00   6.20-14.70     9   (5)
   371016      VINCENT
 09/22/1971 Log-likelihood                  Boat shocking with boom
  Rainbow Trout      370þ 70.10   319.0    14.10     1.41   6.00-21.90    17   (7)
   371012      VINCENT,D
  Brown Trout       1843þ139.50  1326.6    15.30     1.83   5.00-20.90    83   (7)
   371012      VINCENT,D
 04/26/1972 Peterson mark-recapture         Boat shocking - mobile anode
  Rainbow Trout      379þ  0.00   273.0    11.90     0.70   6.80-18.90    27   (5)
   372011      VINCENT
  Brown Trout        912þ  0.00   492.0    15.10     1.50   5.60-23.90    68   (5)
   372011      VINCENT
                      71þ  0.00    32.0     0.00     0.00   7.70-14.90     5   (5)
   372011      VINCENT
 09/05/1972
  Rainbow Trout       67þ  0.00    47.0    12.80     0.93   6.10-18.90     4   (5)
   372012      VINCENT
  Brown Trout       1757þ  0.00   682.0    13.80     1.31   5.50-24.90    92   (5)
   372012      VINCENT
                      90þ  0.00    74.0     0.00     0.00   9.30-16.90     4   (5)
   372012      VINCENT
 09/29/1972 Log-likelihood                  Boat shocking with boom
  Rainbow Trout      299þ100.90   212.0    14.40     1.24   7.00-20.90     5   (7)
   372006      VINCENT,D
  Brown Trout       1563þ214.00  1280.0    14.20     1.41   6.00-22.90    85   (7)
   372006      VINCENT,D
 04/26/1973 Peterson mark-recapture         Boat shocking - mobile anode
  Rainbow Trout      202þ  0.00   127.0    12.70     0.83   6.40-18.90    13   (5)
   373019      VINCENT
  Brown Trout       1317þ  0.00   691.0    15.40     1.65   5.90-24.90    87   (5)
   373019      VINCENT
 09/13/1973
  Brown Trout       1662þ  0.00   823.0    13.70     1.23   5.90-20.90    86   (5)
   373020      VINCENT
 09/20/1973 Log-likelihood                  Boat shocking with boom
  Rainbow Trout      861þ241.10   356.0    11.40     0.73   6.00-19.90    15   (7)
   373021      VINCENT,D
  Brown Trout       1637þ148.60   883.0    14.10     1.62   6.00-21.90    85   (7)
   373021      VINCENT,D
```

if the site were sufficiently homogeneous that a single value could meaningfully convey conditions throughout the site, it is not clear to me that biomass is a useful way to capture the quality of fishing for an angler. One reason has already been alluded to: because of differences in fish size and other characteristics, biomass values are not necessarily comparable across species. Even for a single species of fish, biomass may be an imperfect and unreliable measure of the quality of a site. It may be a good indicator at the extremes -- if there is no biomass fishing, will be bad; if the biomass is huge, fishing cannot fail to be good. But, for intermediate conditions, it may not be a good indicator of fishing quality; it may not even be positively correlated with fishing quality. At any rate, this correlation needs to be demonstrated before one can have great faith in trout biomass as the sole measure of fishing quality at the RUM sites.

Given these conclusions, what are the implications for the estimation of the RUM model and the calculation of lost consumer's surplus in the Upper Clark Fork River Basin? In my view, there are two distinct problems with the TER's biomass variable. First, as I have just indicated, even if biomass were measured well, it may not capture all the aspects of a site that render it desirable for fishing, or all the aspects of a site that were harmed by the release of hazardous substances in the Upper Clark Fork River Basin. Second, TER's biomass variable is *not* measured well, and the measurement error introduces bias into the RUM model. In the following section, I examine the bias from measurement error. In section 4.6, I consider the possibility that there is another variable that captures some of the effects of the release of hazardous substances in the Upper Clark Fork River Basin.

4.5 Monte Carlo Simulation of Measurement Error

Rather than trying to improve their biomass measure or to find a better site quality variable, the TER research team responded to the information that it was based on unreliable and out-dated information by conducting a Monte Carlo simulation experiment intended to bound the effects of the measurement error. From the simulation experiment they concluded that, with 100% measurement error in a variable in a nested logit model, the estimated coefficient on the variable is attenuated by only 28.1% below the true value. By regressing their biomass variable on some stock data, they concluded that the error in the MRIS measure of biomass corresponded to a noise level of 70% (i.e., the error in the variable had a mean of zero and a standard deviation equal to 0.7 of true value of the variable). They concluded that this implied the estimated coefficient of biomass in their RUM model was biased downwards by 19%. Therefore, they raised the fitted coefficient by this percentage when using the fitted RUM model to calculate the loss of consumer's surplus from pollution in the Upper Clark Fork River Basin.

Each link in this chain of reasoning is highly questionable. If the TER research team had simply bothered to inspect the new MRIS database they would have discovered that the measurement error in the biomass variable could be larger than 70%. And, if they had bothered to review the extensive statistical literature on measurement error in logit and related nonlinear models, they would have discovered that the bias in the estimated coefficient could be many times larger than 28%. Indeed, they would have seen this if they had merely looked at their own

results when estimating different versions of the RUM model.

A distinctive feature of the entire section on measurement error in the TER Report is the lack of detail, both in describing the experiment and in relating it to the existing literature. While the Report provides the results of the experiment, it omits key details of the experiment's design, including the number of observations in the sample, the true parameters of the model and the assumed distributions of the variables. Some of these details are mentioned in an internal TER document obtained through the discovery process, entitled "Effects of Data Conditioning, Sample Design, and Aggregation on Random Utility Model Estimates: Some Monte Carlo Results" by Steven M. Waters, F. Reed Johnson and Robert B. Fowler. However, even this document omits some important details. It is not clear, for example, whether the RUM models estimated in the Monte Carlo experiment fit the simulated data as well (or poorly) as the RUM model in the TER Report fits the data from the MOR survey. Nor is it clear whether the distribution of sites shares in the Monte Carlo simulation is the same as that in the MOR survey.[15] One needs some assurance that the model used in a Monte Carlo simulation matches the essential features of the model estimated in the TER Report before one can place much weight on its results.

One also wants to be sure that the results of the Monte Carlo simulation are consistent with the existing literature on measurement error. The Report makes *no* reference to the literature, and there is no evidence from the internal documentation that the TER research team was aware of it when designing the Monte Carlo experiment. This is a serious oversight, since the literature is extensive.[16] It reaches conclusions substantially different from those in the TER Report. It shows that measurement error can "wreak havoc" in model estimation (Carroll, 1989). Carroll, Ruppert and Stefanski (1995) provide a variety of empirical examples where measurement error in logit models attenuates coefficients far more seriously than indicated in the TER Report.

In linear models, where measurement error was studied extensively in the 1960s and 1970s, there are some standard results about the effects of measurement error on regression coefficients estimated by ordinary least squares. The simplest case is where there is measurement error in one of the variables, say X, and the error is additive and uncorrelated with either the other variables in the model or the error in the regression equation. Thus, instead of X one observes W = X + e. When one regresses the dependent variable on W, the result is a consistent estimate not of β (the true regression coefficient for X) but rather of $\gamma\beta$, where γ is given by

$$(1) \qquad \gamma \equiv \sigma_x^2 / \sigma_w^2 = \sigma_x^2 / (\sigma_x^2 + \sigma_e^2) < 1,$$

where σ_x^2 is the variance of the true variable X, σ_w^2 is the variance of the observed variable W, and σ_e^2 is the variance of the measurement error. The estimated coefficient is attenuated by the

[15] I have not had access to the computer code and raw data from the Monte Carlo study, and therefore I am not in a position either to resolve these questions or to verify the accuracy of the results in the TER Report.

[16] A recent book on the subject lists over 300 items in statistics, biometrics and econometrics, most of them written within the last fifteen years (Carroll, Ruppert and Stefanski, 1995).

factor γ. Fuller (1987) calls γ the *reliability ratio.* One can also think of (σ_e^2/σ_x^2) as the *noise-to-signal ratio.*

An important finding of research in the last ten years is that this results holds up rather well in many *nonlinear* models, including logit and related models estimated by maximum likelihood or other methods. This fact has been exploited by various researchers in order to develop corrections for the effects of measurement error (Carrol, Ruppert and Stefanski, 1995). These researchers often use Monte Carlo simulation experiments to test the efficacy of their proposed correction. I have examined these Monte Carlo simulations and found that using γ as an attenuation factor gives a good prediction of the effects of uncorrected measurement error in logit models. This holds for the Monte Carlo simulations in Stefanski (1985), who uses 300 and 600 replications per experiment; Schafer (1987), who uses 100 and 300 replications per experiment; Whitemore and Keller (1988), who use 50, 100, and 500 replications per experiment; and Rosner, Willett and Spiegelman (1989), who use 1,000 replications per experiment. In each case, the researcher constructs the Monte Carlo experiment by first picking a distribution for the true value X with some given mean and variance, and then picking a distribution for e with a mean of zero and a variance σ_e^2 that is some specified proportion of σ_x^2. For example, in successive experiments they may set $\sigma_e^2 = 0.1\sigma_x^2$, $1.05\sigma_x^2$, $1.5\sigma_x^2$, and $2\sigma_x^2$, which generates attenuation factors of $\gamma = 0.909$, 0.5, 0.4 and 0.333, respectively. I found that these factors generally predict the Monte Carlo simulation results well, but they tend to underestimate the attenuation from measurement error when the distribution of e has heavier tails than a normal distribution (Stefanski, 1985; Schafer, 1987).

The TER research team was clearly unaware of this literature when they designed their own Monte Carlo experiment, because they constructed it in a rather artless manner. They picked a distribution for X with some mean, μ_x, and variance σ_x^2, and then specified a distribution for e with a mean of zero and a standard deviation σ_e that is some specified proportion of μ_x rather than σ_x. For the distribution of X, they used a uniform distribution with a lower support at zero. Regardless of the upper support, this type of uniform distribution has a coefficient of variation of $\sigma_x/\mu_x = 3^{-0.5} = 0.5774$; i.e., $\sigma_x^2 = \mu_x^2/3$. For the distribution of e, they used a normal with a mean of zero and a standard deviation of $\sigma_e = \theta\mu_x$ where they varied θ over a range from 0.1 to 1.0. This corresponds to an attenuation factor of γ where

$$(2)\qquad \begin{aligned}\gamma &= (\mu_x^2/3)\ /\ [(\mu_x^2/3) + (\theta^2\mu_x^2)]\\ &= (1/3)\ /\ [(1/3) + \theta^2].\end{aligned}$$

When $\theta = 1$, $\gamma = 0.25$. When $\theta = 0.7$, which TER research team considers to hold for their biomass variable, $\gamma = 0.409$ and the regression coefficient is understated by about 59%.

However, the Monte Carlo simulations in the TER Report show much less attenuation -- they imply attenuation factors (γ-values) of .719 when $\theta = 1$, and .81 when $\theta = 0.7$. In other words, while there is an established body of results in the published literature supporting the finding that a measurement error in a logit model corresponding to $\theta = 0.7$ would bias the

coefficient of the affected variable by 59%, the TER study finds this bias to be only 19%. This is a 300% difference.

What can account for such inconsistent results? One possibility is that the result in the TER study is caused by a relatively small sample size -- the TER simulation uses only 100 replications per experiment, significantly less than the studies mentioned above. Another possible explanation is that there might be some confounding in the data -- if there were some unintended correlation between the error term, e, and the other stochastic components of the model, or between the variable measured with error and the other covariates, this certainly would affect the degree of attenuation. Since I do not have access to the data used in the TER Monte Carlo study, I cannot check this. There is also the chance of programming error -- it is my observation that the computer programming by TER research team tends to be rather sloppy and sometimes careless; in one case, discussed below, we have found a significant error. Since I have not seen the programs for the Monte Carlo study, I cannot rule this out.

A third explanation rests on a modification to the Monte Carlo experiment which is mentioned in a footnote in the TER Report. This states: "if the noise added to a site characteristic produced a negative number, a level of zero was substituted." I have not seen this done in any of Monte Carlo studies in the published literature. It reduces the effective variance of the random error, e, below what is nominally specified. The reason why the TER research team took this unusual action is that there would otherwise have been a negative value for the explanatory variable in the logit experiment. They wished to rule this out because they wanted the variable to stand in for biomass (although it does not have a distribution in the experiment that looks anything like the distribution of biomass as shown in Table 8). But, the real implication to be drawn from the occurrence of negative values of (X+e) is that the assumption of an *additive* error of measurement is inappropriate. This is certainly what the data in Table 8 suggest -- the measurement error is more likely to be multiplicative. The reason for this is that a false reading on the downside shrinks the true value of the biomass, but with a bound of zero, while a false reading on the upside raises it without any similar bound. Either the simulation should be conducted with a multiplicative error or else, if one uses an additive error, the negative values should be left in the data. This is what the other researchers did. What the TER research team did reduces the effective variance of the measurement error, at a rate that increases with θ, which undermines the purpose of the simulation experiment.

A fourth possible explanation concerns the type of logit model used in the study. The Monte Carlo simulations of measurement error published in the literature deal with a non-nested logit model estimated by maximum likelihood; the simulation in the TER Report involves a two-level, nested logit model estimated by full information maximum likelihood (FIML). The measurement error affects only a variable in one of the lower branches at the lower level. It is possible that when a nested model is estimated by FIML, this mitigates the effect of measurement error. The fact that the rest of the tree, which is free from measurement error, is estimated simultaneously with the part of the tree which has the measurement error may dampen the effects of the error. If so, this would be an interesting finding. But, it would *not* apply to the estimates of lost consumer's surplus in the Upper Clark Fork River Basin, since those are based on a

nested logit model in the TER Report which is estimated with sequential maximum likelihood (SML). The SML estimation of the part of the tree affected by measurement error -- i.e. the choice among river sites based involving the biomass variable -- is the same as a simple, non-nested logit. Therefore, the Monte Carlo results in the existing literature, rather than those in the TER simulation, would apply in this case.

In short, on the basis of my analysis in Section 4.4, I believe that the biomass variable in the TER RUM model is infected with substantial measurement error. Based on my analysis in this section, I believe that this measurement error can have a substantial impact on the coefficient of biomass which is used in the TER Report to calculate lost consumer's surplus resulting from the release of hazardous substances in the Upper Clark Fork Basin. If the measurement error corresponds to a value of $\theta = 0.7$, as the TER Report maintains, following the published literature I believe that this would attenuate the coefficient of biomass in SML estimation by about 59%.

Two other points should be noted. First, there may be corroboration of the impact of measurement error from the so-called "elemental site model" in the TER Report, in which fishing sites are defined at a more disaggregated level than the RUM sites considered so far. Instead of 59 RUM river sites and 38 RUM lake sites, there are 216 elemental river sites and 93 elemental lake sites. The difference is that, whereas the RUM sites correspond to several reaches of a river or set of rivers, the elemental sites correspond to individual reaches of a river. The TER Report presents a model of fishing participation and site/duration choice for these sites, involving trout biomass among the determinants of choice for river sites. The biomass measure for the elemental river sites is taken from the 1989 version of the MRIS database. Instead of having to average biomass values over different portions of the RUM site, the TER research team uses the specific biomass value in MRIS for the reach which constitutes the elemental site.

I have not had the opportunity to see either the data or the computer code used in the elemental model. However, I would imagine that the measurement error in the biomass values for elemental sites is more severe than for RUM sites. Normally, aggregation of heterogeneous sites is undesirable -- indeed, this is a criticism of the RUM sites. However, to the extent that measurement errors in biomass cancel out when one aggregates individual sites, aggregation could reduce error in a variable measured so poorly as biomass. Therefore, I would expect the attenuation of the coefficient on biomass to become far worse in the elemental model than the RUM model. This is consistent with what happens -- the coefficient on biomass falls by a factor of 138, from 0.1681 in the RUM model to 0.0012165 in the elemental model. The TER Report neither comments on this nor offers an explanation. My guess is that it is a consequence of a drastic decline in the reliability ratio for the biomass variable.

Second, measurement error in one variable invariably affects the coefficients of other variables even if they are measured without error and are statistically independent of the variable with the measurement error. This arises because of what might be called the "perfect prediction" property. In the case of a linear model, by construction the ordinary least squares regression line passes through the point of means: at the mean value of the explanatory variable, the fitted

equation must correctly predict the mean value of the dependent variable, regardless of any measurement error or any misspecification in the model. Thus, at the means, the OLS estimates perfectly fit the data. An analogous condition holds for logit models with constant terms or dummy variables: as will be discussed below, by construction maximum likelihood estimates of these models perfectly predict the actual shares of the alternatives in the data. For other nonlinear models like the nested logit model estimated by FIML in TER's Monte Carlo simulation, similar conditions hold on average if not exactly. By construction in these models, on average the fitted model must predict the actual shares regardless of measurement or specification error. Consequently, when measurement error in one variable throws that coefficient out of kilter, the coefficients of the other variables have to adjust in an offsetting direction in order for the model to continue on average to predict perfectly. Thus, the coefficients of all variables can be affected by measurement error in just one variable. However, they are affected differently. The coefficient on the variable measured with error is attenuated towards zero -- i.e., it falls if positive, and rises if negative. If there is but one other variable, as in the TER Monte Carlo experiment, its coefficient must move in the *opposite* direction -- i.e., away from zero -- in order to satisfy the perfect prediction property. The TER research team observe the change in the coefficient on cost in their Monte Carlo experiments, but appear not to grasp the reason for its direction.

They do note an important implication for the measurement of consumer's surplus. Since the consumer's surplus associated with a change in biomass depends essentially on the ratio of the coefficient on biomass divided by the coefficient on cost, measurement error has a double impact: if lowers the numerator and may also raise the denominator. Thus, it can attenuate the estimate of consumer's surplus disproportionally more than it attenuates the coefficient on biomass. For example, when $\theta = 1$, the TER Report (page 79) notes that while the Monte Carlo simulations show a 28.1% decrease in the coefficient on biomass, they show a 45.8% decrease in consumer's surplus per choice occasion. Here, the attenuation of consumer's surplus is 63% larger than the attenuation of the coefficient on biomass.

It is all the more surprising that TER research team proceed to overlook this double effect of measurement error in Section 8.3 of the Report, where they calculate the loss of consumer's surplus from recreation in the Upper Clark Fork River Basin. In that section, they allow for the attenuation in the coefficient of biomass (inadequately, as I have argued above), but they ignore the potential impact on the coefficient of travel cost which would generate a disproportionally larger attenuation of consumer' surplus. I suppose they hoped that nobody would remember what they had written on page 79.

4.6 Other Site Characteristics

I noted earlier that, in addition to biomass, the RUM choice model for river sites includes a variable AESTHETICS. This is a dummy variable which is coded "1" or "0" (the values for the river sites are shown in the first column of numbers in Table 9). The Report describes it as an "aesthetics rating for rivers" and says that it comes from the MRIS database. I checked the 1989 MRIS database and found that, in the Fishery Assessment section, there is a something

called "esthetics rating" which is coded as "outstanding," "above average," "average," "below average," "low," and "national renown." These codings are shown in the second column of numbers in Table 9. Since RUM sites generally consist of several stream sections, sometimes quite heterogeneous in quality, there can be several different esthetics ratings associated with a single site. In those cases, I have listed the different ratings separately.[17] The general rule seems to be that the TER researchers coded a site as 1 in their AESTHETICS variable if it had an MRIS esthetics rating of 4, and 0 otherwise. However, there are some exceptions to this rule, such as sites 52, 33, 35 and 41. For comparison, I have included the TER biomass variable in the last column of Table 9, and I have sorted the sites by the MRIS esthetics rating. While all the sites with the highest biomass values have esthetics ratings of 4, there are some sites with esthetics ratings of 4 that have distinctly low biomass values like the Kootenai River (site 1) and the Blackfoot River (site 22). The biomass value for Dearborn River (site 16) also seems very anomalous. The juxtaposition of biomass values with the other ratings in the table drives home the peculiarity of the biomass variable.

I wondered when I saw the categories for the esthetics rating because, in my experience of studying fishing, the term "river of national renown" is used synonymously with terms like "blue-ribbon trout stream" or "trophy trout stream" to indicate excellence of fishing, not scenery. To investigate the matter further, I consulted a trout fishing book, *Trout* by Dick Sternberg, a volume in the Hunting & Fishing Library published by Cy DeCosse Incorporated in 1988. This book lists blue-ribbon trout streams across the country. The streams in Montana that it lists coincide pretty well with the streams that are rated "national renown" in the MRIS database and that have a TER AESTHETICS value of 1 (see Table 9).

To resolve the matter, I contacted George Holton in Helena, who is a retired Assistant Division Administrator in Montana DFWP. He was the Manager of the Interagency Stream and Fisheries Database, of which MRIS is part. He was kind enough to send me a document entitled "Method for Assessing the Significance of River Segments and Systems for Fisheries Resources in Montana," dated November 11, 1985, which contains the instructions for coding the Esthetics rating and other components of the Fishery Assessment Section of the MRIS database. These instructions are reproduced in Figure 3. There are instructions for both a letter rating, A - F, and a numerical code, 0 - 4, where 0 is low and 4 high. Footnote 10 refers to an explanation of the esthetics ratings in Attachment. This explanation is reproduced in the lower half of the figure. It is evident from this that, while the middle ratings (A, B, C; 3, 2, 1) of esthetics measure scenic beauty, the highest rating (F; 4) and the lowest rating (D, E; 0) measure factors associated with trout productivity and water quality -- factors directly influenced by releases of hazardous substances in the Upper Clark Fork River Basin. Since the TER AESTHETICS variable picks out the sites with an esthetics rating of 4, it serves to identify the most productive trout streams in the state, not necessarily the most scenic. Moreover, it does so in a more robust manner than the biomass variable.

[17] In the MRIS database, there is no esthetics rating for Silver Bow Creek; I assume it was off the scale at the low end. For the sake of completeness, I have inserted a rating of 0 in the table.

TABLE 9: SITE CHARACTERISTIC VARIABLES FOR RUM SITES

RUM SITE	SITE NAME (TER)	TER AESTHETICS	MRIS ESTHETICS (1)	BLUE-RIBBON TROUT (2)	MRIS SCENIC (3)	TER BIOMASS
5	Flathead River	1	4	1	1.7	157
3	Upper Flathead River	1	4	1	3	70.94
57	Missouri River (Five Lakes)	1	4	1	3	590
52	Missouri River (Fort Benton)	0	4	1	2	111.07
38	Madison River	1	4	1	3.5	315.95
44	Middle Yellowstone River	1	4	1	3	110.48
43	Upper Yellowstone River	1	4	1	3.5	118.95
1	Kootenai River	1	4, 1	1	4	86.02
21	Rock Creek	1	4, 3	1	4	166.72
39	Missouri River	1	4, 2	1	2	243.43
42	Upper Gallatin River	1	4, 2	1	3.5	167.13
34	Lower Big Hole River	1	4, 2	1	3.5	137.41
53	Bighorn River	1	4, 0	1	3	362.02
17	Lower Missouri River	1	4, 2, 1, 0	1	2.7	234.62
22	Blackfoot River	1	4, 2, 1	1	2.5	69.69
33	Upper Big Hole River	0	4, 2, 1	1	2.5	55.78
13	South Fork Flathead River	0	4, 3, 1	0	3	9
40	Upper Madison River	1	4, 1, 2	1	4	363.8
41	East & Lower Gallatin Rivers	0	4, 1	1	2.5	99.88
35	Beaverhead River	0	4, 1, 0	1	2.5	260.24
16	Dearborn River	1	3, 2	0	3	18.7
6	Middle Fork Flathead River	0	3, 2	1	4	78
18	Smith River	0	3, 2, 1, 0	0	2	63.37
19	Belt Creek	0	3, 1	0	2.2	13.7
45	Boulder River	0	2	0	3	67.06
29	Boulder River	0	2, 1	0	2	22.8
25	Flint Creek	0	2, 1	0	3	67.24
36	Ruby River	0	2, 1	0	3.3	52.82
30	Judith River	0	2, 1	0	2.3	6.11
46	Stillwater River	0	2, 1	0	3	69.59
28	Tenmile & Prickly Pear Creeks	0	2, 1, 0	0	1.8	49.85
27	Little Blackfoot River	0	2, 0	0	2.8	50.05
24	Clark Fork River (Warm Springs-Garrison)	0	1	0	2.5	94.28
37	Jefferson River	0	1	0	3	100.8
14	Teton River	0	1	0	2	15.78
32	Upper Bitterroot River	0	1	0	3	73.32
23	Clark Fork River (Garrison-Milltown)	0	1	1	3	29.93
31	Musselshell River	0	1	0	2.5	60.4
20	Lower Bitterroot River	0	1	0	3	123.07
2	Yaak River	0	1	0	3	8.66
12	Swan River	0	1	1	2	90
9	St. Regis River	0	1	0	2.5	46.61
10	Flathead River	0	1	0	2.5	69.93
11	Middle Clark Fork River	0	1	0	2.3	56.77
47	Red Rock River	0	1	0	2.5	68.82
54	Clarks Fork Yellowstone River	0	1	0	2.2	47.19
50	Lower Yellowstone River	0	1	0	2.5	155.49
48	Grasshopper & Prairie Creeks	0	1, 0	0	2.9	14.55
15	Sun River	0	1, 0	0	1	28.68
59	Ashley Creek	0	1, 0	0	1	3.42
58	Red Lodge & Willow Creeks	0	1, 0	0	1.3	6
8	Lower Clark Fork	0	1, 0	0	1.5	56.59
4	Stillwater & Whitefish Rivers	0	1, 0	0	1.75	12.79
7	St. Mary River etc	0	1, 0	0	2.5	24.48
55	Yellowstone River (Billings)	0	1, 0	0	2.2	39.56
56	Bearpaw Mountains	0	1, 0	0	2	28.29
51	Silver Bow Creek	0	0	0	1	0
26	Warm Springs Creek	0	0	0	2	30
49	Little Bitterroot River	0	0	0	3	22

NOTES: (1) 4 = national renown; 3 = outstanding; 2 = above average; 1 = average; 0 = below average, low.
(2) 1 = listed as blue-ribbon trout stream in Hunting & Fishing Library volume on Trout; 0 = not so listed.
(3) 4 = outstanding (excellent); 3 = substantial (high); 2 = moderate; 1 = limited.

FIGURE 3: INSTRUCTIONS FOR ASSIGNING AESTHETICS GRADE IN MRIS DATABASE

Criterion III. Assignment of Esthetics Grade

Esthetics rating [10/]	Grade
A	3
B	2
C	1
D	0
E	0
F	4

[10/] See explanation of esthetics ratings in Attachment.

ESTHETICS RATINGS. Esthetics are rated A (high) through E (low). Features that detract from esthetics include: pollution, dewatering, channelization, riprap (particularly car bodies and discarded building materials), mine tailings, a busy highway along stream and severe land abuse. As a guide:

A A stream of outstanding natural beauty in a pristine setting.

B A stream comparable to A except that it may lack pristine characteristics. Presence of human development such as roads, farms, etc., usually comprise the difference between B and A.

C A stream with natural beauty but of a more common type than listed under A and B. A clean stream in an attractive setting.

D A stream and area with fair esthetics.

E A stream with low esthetics.

F A stream of national renown.

If one wants a measure of scenic quality, this is provided by a variable in the Recreation Assessment of the MRIS database called "Scenic Quality". This rates reaches of rivers using 5 categories: 1 - Exceptional; 2 - High; 3 - Moderate; 4 - Limited; and 0 - Unknown. Figure 4 presents descriptions of these categories, taken from Appendix E of the current MRIS manual, *Montana Rivers Information System: Reporting User's Manual*, February 1994. The scenic quality ratings for the RUM sites are shown in the penultimate column of Table 9; in this case I have taken a simple, unweighted average of the ratings for the individual stream segments that make up a RUM site. While many scenic sites also rank high on the esthetics ratings, and conversely for unattractive sites, there are important cases of divergence. The Little Bitterroot River (site 49) has a high rating for scenic quality, but a low one for esthetics. Conversely, site 52 (Missouri River, Fort Benton) has a high rating on esthetics but a relatively low one for scenic quality. Similarly with respect to AESTHETICS. This reinforces my conclusion that the AESTHETICS variable in the TER Report is measuring trout productivity rather than scenic beauty.

FIGURE 4: CODING OF SCENIC QUALITY IN MRIS DATABASE

LOOKUP TABLE VI - SCENIC QUALITY

1=EXCEPTIONAL
Outstanding scenic quality. For these segments, landforms, vegetation patterns, and water features combine to create unique, highly memorable, and harmonious visual settings. Views along the river and away from the river to surrounding scenery are highly diverse, providing river users with scenery that is spectacular and/or not common on other rivers in the region. If buildings, roads, and other cultural modifications are present, they either add favorably to or do not intrude on visual quality for river users.

2=HIGH
High scenic quality. For these segments, landforms, vegetation patterns, and water features combine to create a highly memorable and visually pleasing setting, although one that may be more common to the region. Views along and away from the river are highly diverse and cultural modifications, if present, either add to or do not detract from the visual setting.

3=MODERATE
Moderate scenic quality. For these segments, landforms, vegetation patterns, and water features along the river combine to create harmonious but common visual settings. Views along and away from the river are somewhat varied, but lack a high degree of contrast and diversity. Encroachment of cultural modifications may be evident, and either adds little to or detracts from visual quality.

4=LIMITED
Low scenic quality. For these segments, landforms, vegetation patterns, and water features combine to create visual settings lacking in variety and contrast. Views along and away from the river are monotonous and common. Cultural modifications may dominate and detract from visual quality.

0=UNKNOWN
Insufficient information available to evaluate scenic quality.

5. The RUM Model: Structure and Estimation

5.1 Overview

The RUM model developed by the TER research team operates on a weekly time-step, with three nested levels of angler choice: (1) Whether to take a fishing trip this week and, if so, how many trips (1 trip vs. more than 1 trips)? (2) Given that the angler is taking a trip this week, should he go to a river or lake site? (3) Given the type of site, to which specific river or lake site should he go, and for how long (1 day, 2 days, 3 days, or 4+ days)?

The general type of structure model is not unreasonable. Indeed, as the Report notes, it is patterned after a model that I developed with Richard Carson for sportfishing in Southcentral Alaska in 1986, and a related model that Richard and I developed for sportfishing in Southeastern Alaska in 1988, on which Steven Waters worked as our research assistant and which served as the basis for his Ph.D dissertation under Richard at U.C. San Diego. My concerns center on two issues: the limitations of the data, and the specific details of the tree structure.

With regard to the data used to estimate the model, I have identified the following limitations:

RUM DECISION TREE	DATA LIMITATIONS
Choose to recreate? How many trips to take this week: 0, 1 or 2+ trips?	Over-estimation of anglers in the MOR telephone survey, combined with severe under-estimation of the number of fishing trips due to progressively increasing nonreporting in the MOR panel survey, make it impossible to obtain a reliable estimate of this choice.
River or lake site for this trip?	Over-estimation of trips in the MOR survey to river sites in summer, and under-estimation in winter, make this unreliable.
To which lake/river site, and how long a trip (1, 2, 3, or 4+ days)?	The nonreporting in the MOR survey gives rise to a tiny number of observations relative to the large number of sites. Oversampling of long trips and of avid anglers, and inclusion of individuals who are outliers, bias results. Errors in measuring variables bias coefficient estimates.

I observed at the beginning of this report that any economic model, no matter how excellent in theory, will be of little practical value if it is applied to unreliable, unrepresentative or contaminated data. I have come to the conclusion that the TER model of angler behavior falls into this category. In my view, the data from the MOR survey are not reliable for estimating the top two tiers of the model. Those two tiers should be dropped from the model, and the choices in those tiers should simply be benchmarked against the far more extensive and reliable data from the National Survey of FHWAR and the Statewide Angling Pressure Survey. For reasons spelled out below, I am not certain at this time about the reliability of the data used for the bottom tier of the model. I am sure, though, that these data are inconsistent with the specific structure of the TER model.

In this section I discuss three aspects of the model: the assumptions implicit in the pooling of data used to estimate the model, the assumptions implicit in the model structure, and the results when the model is estimated.

5.2 Pooling Disparate Data

One of the most striking features of TER's RUM model is the contrast between the small number of respondents in the MOR survey reporting their fishing trips, on the one hand, and the relatively large number of sites that the TER research team chose to model -- 97 sites and only 1736 trips, producing an average of only 18 trips per site. Moreover, 165 of the trips come from two individuals who are clearly outliers.

The only way the TER research team could model choices among so many sites with so little data is by making three strong assumptions about angler preferences and behavior:

(A) Winter fishing and summer fishing are sufficiently similar activities that they can be pooled and modelled with a single set of demand functions.

(B) The fishing preferences and behavior of anglers in the different parts of Montana are sufficiently similar that the data for all resident anglers can be pooled and modelled with a single set of demand functions.

(C) All river fishing sites in the TER study area in Montana, and all lake fishing sites, are sufficiently similar, and the explanatory variables included in the RUM model are so comprehensive (and measured with such accuracy), that one "generic" set of demand functions can be fitted for all river sites, and another for all lake sites. The demand functions for all rivers depend on exactly the same explanatory variables, and these variables have exactly the same coefficients, including intercept term, for all rivers. Similarly, the demand functions for all lakes depend on exactly the same explanatory variables, and these variables have exactly the same coefficients.

These assumptions greatly simplified the task of the TER research team. They permitted

it to pool all trips in a single estimation, and they made it possible to estimate demand functions for sites where there were few observations -- indeed, for five RUM sites, where there were *no* observations. All of this was due to the immense power of assumptions. Who needs data when he has assumptions! The TER research team made no attempt to investigate whether these assumptions hold. They took them for granted. Based on my own experience of studying fishing, I doubt that the assumptions hold.

With regard to the pooling of summer and winter fishing trips, while this may be of minor concern in regions with an unchanging climate year-round, such is not the case in Montana. In northern regions, in my experience, winter and summer fishing are very different experiences, and appeal to different kinds of angler. As a colleague once remarked to me when we were studying fishing in Alaska, while the best substitute for fishing at one site in summer is fishing at another site, the best substitute for fishing at one site in winter is drinking in a bar. In our study of Southcentral Alaska we found a considerable difference in patterns of angler behavior and site preference for summer and winter fishing. This does not mean that there is little value from fishing in winter, or that the consumer's surplus from winter fishing is lower for those who participate in it than the consumer's surplus from summer fishing. It is just that these are different activities and, in my judgment, they should be modelled separately.

In this case, however, if the TER research team had separated summer and winter fishing trips, it would have been left with only 1196 summer trips and 540 winter trips with which to account for 97 sites (Table 10). The sample size is probably too small to permit a reliable analysis of winter fishing at the 97 sites.

The data in Table 10 also show why it may be inappropriate to lump anglers from all areas into a single set of demand functions. Not only should the out-of-state angler have been dropped, but also the resident anglers from the North Central Area. Their lake/river choice is radically different from the rest of the sample. I suspect that many of the fishing sites they patronize are among those excluded from the RUM sites -- far more so than for the other anglers in the sample.

Table 11 shows the distribution of fishing trips by anglers in each area, broken down by season. The difference in distribution of trips among individual sites provides strong support for treating winter and summer fishing separately. The table also casts doubt on the reasonableness of lumping all anglers into a single set of demand functions. This is illustrated in Figure 5, which shows the drainages in which Billings area residents took fishing trips, whether winter or summer. Not surprisingly, anglers from this area went fishing at RUM sites in the same general area, RUM sites in surrounding areas, and RUM sites in southwest Montana. They did *not* go to RUM sites in drainages 5, 7, 8, and 11. Conversely, Figure 6 shows that anglers from the Kalispell area did go to those sites, but did not go to RUM sites 1, 9, 13, 17 or 20. Is it reasonable to assume that anglers in each area are choosing from the full set of 97 sites when they go fishing? If so, does every site enter every angler's utility function in a purely generic manner? The TER research team assumes a (convenient) answer to those questions, without establishing that the assumption is correct.

TABLE 10: BREAKDOWN OF MOR TRIPS BY RIVER vs LAKE SITES, BY SEASON & AREA OF RESIDENCE

	SUMMER TRIPS			WINTER TRIPS		
AREA OF RESIDENCE	LAKE	RIVER	% RIVER	LAKE	RIVER	% RIVER
BILLINGS AREA	45	119	72.6%	15	40	72.7%
GREAT FALLS AREA	80	67	45.6%	21	21	50.0%
NORTH CENTRAL AREA	15	3	16.7%	7	2	22.2%
HELENA AREA	98	110	52.9%	86	38	30.6%
SOUTH WEST AREA	123	153	55.4%	84	24	22.2%
MISSOULA AREA	86	147	63.1%	34	39	53.4%
KALISPEL AREA	75	73	49.3%	57	72	55.8%
OUT OF STATE	0	2	100.0%			
TOTAL	522	674	56.4%	304	236	43.7%

TABLE 11: DISTRIBUTION OF TRIPS IN MOR SURVEY, BY SEASON & AREA OF ORIGIN

DFWP DRAINAGE	RUM SITE	SITE NAME (TER)	BILLINGS AREA RESIDENTS WINTER	BILLINGS AREA RESIDENTS SUMMER	GREAT FALLS AREA RESIDENTS WINTER	GREAT FALLS AREA RESIDENTS SUMMER	N CENTRAL AREA RESIDENTS WINTER	N CENTRAL AREA RESIDENTS SUMMER	HELENA AREA RESIDENTS WINTER	HELENA AREA RESIDENTS SUMMER	S WESTERN AREA RESIDENTS WINTER	S WESTERN AREA RESIDENTS SUMMER	MISSOULA AREA RESIDENTS WINTER	MISSOULA AREA RESIDENTS SUMMER	KALISPEL AREA RESIDENTS WINTER	KALISPEL AREA RESIDENTS SUMMER
1	79	Clark canyon reservoir		0.6%	0.0%	0.0%			0.8%		13.0%	3.3%		0.4%		
1	36	Ruby River								0.5%						
1	35	Beaverhead River									1.9%	7.2%				
1	48	Grasshopper & Prairie Creeks														
1	47	Red Rock River										2.2%				
2	33	Upper Big Hole River									0.9%	3.3%		2.1%		
2	34	Lower Big Hole River								1.4%	3.7%	11.6%		0.9%		0.7%
3	20	Lower Bitterroot River										0.4%	6.2%	6.9%		
3	73	Bynum Reservoir			7.1%	3.4%								0.4%		
3	87	Lake Como											1.4%	1.7%		
3	32	Upper Bitterroot River												3.9%		
4	22	Blackfoot River				4.1%				1.9%		0.4%	2.7%	10.7%		0.7%
4	70	Mult-Lake				0.7%			0.8%	1.0%	0.9%		5.5%	5.2%	0.8%	
6	9	St. Regis River											1.4%	2.6%		
5	11	Middle Clark Fork River											8.2%	16.3%		
5	8	Lower Clark Fork								0.5%				3.9%		0.7%
6	61	Bull Lake													2.3%	0.7%
5	68	Cabinet Gorge & Noxon Reservoirs														1.4%
6	61	Silver Bow Creek														
6	21	Rock Creek											8.2%	3.4%		
6	23	Clark Fork River (Garrison-Milltown)		0.6%						1.0%		0.4%	17.8%	3.0%		
6	26	Warm Springs Creek														
6	27	Little Blackfoot River				0.7%				5.3%		0.4%		1.7%		
6	74	Georgetown Lake							0.8%	0.5%	27.8%	21.0%	20.5%	4.3%		
6	24	Clark Fork River (Warm Springs-Garrison)										0.7%				
6	25	Flint Creek								0.5%		0.7%		1.7%		0.7%
7	65	Foy Lake													2.3%	0.7%
7	64	Hungry Horse Reservoir												0.4%		1.4%
7	83	Smith Lake														9.5%
7	69	Flathead Lake				1.4%			1.6%	1.0%	0.9%	1.4%	19.2%	12.9%	10.1%	16.9%
7	49	Little Bitterroot River														
7	5	Flathead River													53.5%	14.2%
7	92	Tally, Sylvia & Lupine Lakes				0.7%										2.7%
7	89	Holland, Lindberg & Glacier Lakes												2.6%		0.0%
7	62	Whitefish Lake													0.8%	0.7%
7	63	Ashley & Little Bitterroot Lakes													7.8%	4.1%
7	4	Stillwater & Whitefish Rivers													1.6%	4.1%
7	10	Flathead River											6.8%	2.6%		
7	96	Van Lake etc												0.9%		2.7%
7	66	Swan Lake				0.7%				0.5%		0.4%			0.8%	2.0%
7	12	Swan River												0.9%		1.4%
7	59	Ashley Creek												0.4%		0.7%
8	6	Middle Fork Flathead River								0.5%						2.0%
8	13	South Fork Flathead River								0.5%				0.4%		0.7%
8	3	Upper Flathead River				0.7%										3.4%
9	42	Upper Gallatin River										4.0%				
9	38	Madison River							1.6%		0.9%	4.3%				
9	41	East & Lower Gallatin Rivers							0.8%	1.0%	3.7%	6.5%				
10	95	Delmoe Lake									1.9%	1.1%				
10	37	Jefferson River							3.2%	1.8%	3.7%	4.7%				
10	29	Boulder River								3.4%		2.2%				
11	60	Lake Koocanusa													14.7%	6.8%
11	81	Thompson Lakes													4.7%	1.4%
11	1	Kootenai River												0.9%	0.8%	18.9%
11	2	Yaak River														1.4%
13	40	Upper Madison River		0.6%						1.4%		0.4%				
13	80	Hebgen Reservoir		0.6%						0.5%		0.7%				
13	78	Ennis Lake									3.7%	0.7%				
14	14	Teton River				1.4%										
14	67	Lake Frances			9.5%	10.2%				1.0%				1.3%		
14	7	St. Mary River etc			4.8%	4.8%				0.5%						
14	85	Lake Elwell			2.4%	6.2%		16.7%				0.4%				
16	94	Beaver Creek & Bearpaw Reservoirs					33.3%	33.3%								
15	56	Bearpaw Mountains		0.6%			22.2%	18.7%								
16	86	Ackley Lake			2.4%	4.1%										
16	30	Judith River				4.1%	-									
17	52	Missouri River (Fort Benton)				0.7%						0.4%				
17	19	Belt Creek				11.6%										
17	18	Smith River		1.2%	7.1%	6.8%				1.9%		0.4%				
17	16	Dearborn River														
17	57	Missouri River (Five Lakes)							8.9%	4.8%				0.4%		
17	17	Lower Missouri River			35.7%	6.8%			8.9%	14.9%	0.9%	1.4%		0.4%		
17	75	Five Lakes	1.8%	1.2%	7.1%	11.8%	11.1%		34.7%	30.3%	6.3%	6.2%		5.6%		
17	39	Missouri River							7.3%	7.7%	5.6%	1.1%				
17	91	Newlan Creek Reservoir				0.7%				1.4%		0.4%				
17	26	Tenmile & Prickly Pear Creeks								3.4%						
17	76	Canyon Ferry Reservoir		2.4%		0.7%	33.3%	33.3%	29.8%	10.1%	10.2%	6.3%		1.3%		
18	90	Martinsdale Reservoir	7.3%	1.8%								0.4%				
18	31	Musselshell River														
19	66	Lower St. Mary Lake			4.8%	0.7%				0.5%						
20	15	Sun River				1.4%										
20	77	Willow Creek Reservoir							0.8%		11.1%	0.4%				
20	72	Mult-Reservoir			14.3%	9.5%				0.5%						
20	71	Gibson reservoir		0.6%	2.4%	2.0%										
22	43	Upper Yellowstone River		3.7%							0.0%	1.1%				
22	44	Middle Yellowstone River	30.9%	15.9%							0.9%	1.4%				
22	97	Emerald Lake		3.0%												
22	93	Greenough Lake		1.2%												
22	45	Boulder River	3.6%	5.5%		0.7%										
22	58	Red Lodge & Willow Creeks	3.6%									0.4%				
22	53	Bighorn River	1.8%	7.3%												
22	55	Yellowstone River (Billings)	3.6%	11.8%												
22	54	Clarks Fork Yellowstone River	5.5%	3.0%	2.4%											
22	82	Cooney Reservoir	14.5%	13.4%												
22	46	Stillwater River	20.0%	10.4%												
22	84	Bighorn Lake	3.6%	2.4%												
22	50	Lower Yellowstone River	3.8%	12.2%												
		TOTAL	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%	100.0%
		NUMBER OF TRIPS	55	164	42	147	9	18	124	208	106	276	73	233	129	148

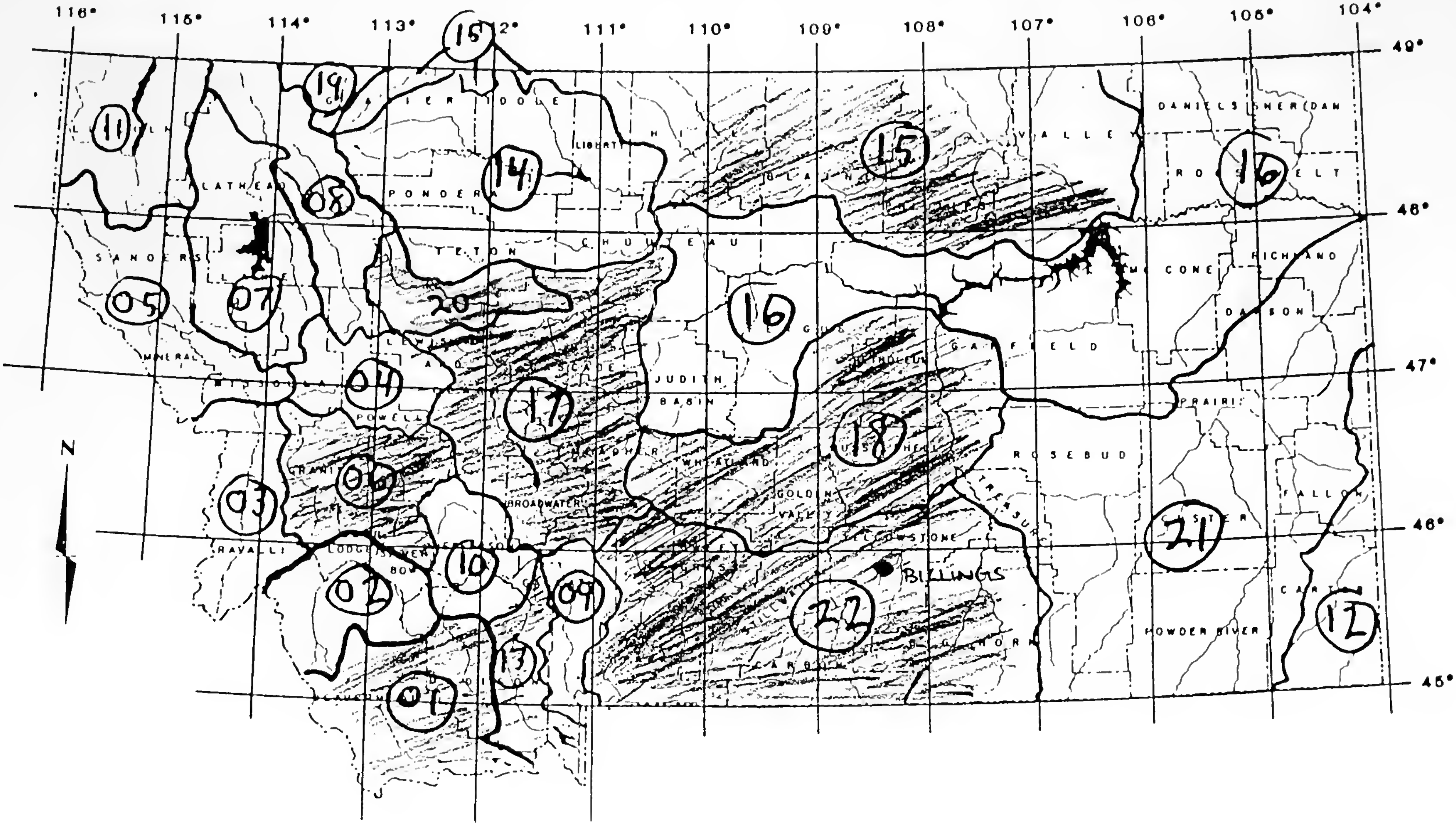


FIGURE 5: REGIONS WHERE ANGLERS FROM THE BILLINGS AREA FISHED

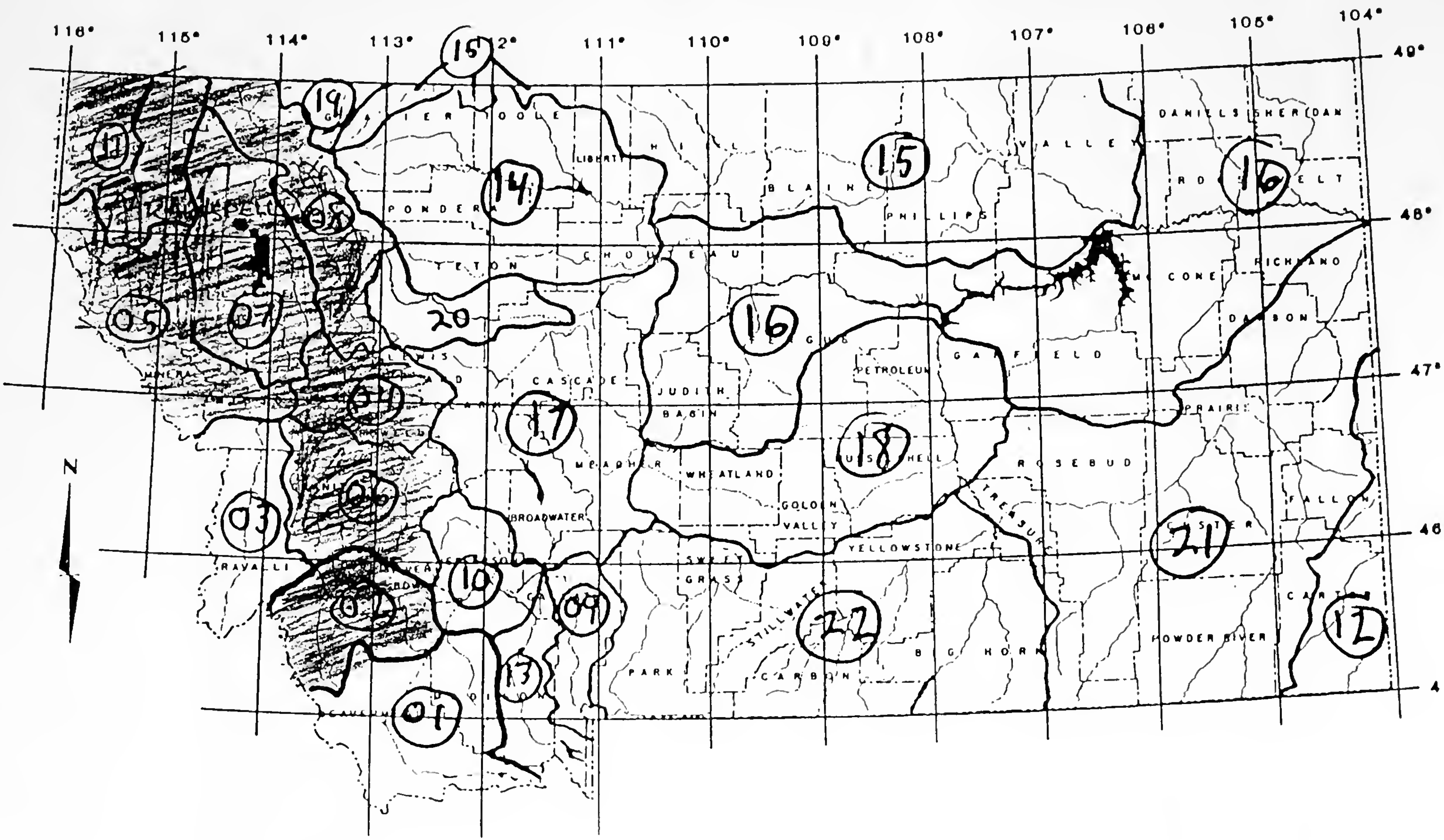


FIGURE 6: REGIONS WHERE ANGLERS FROM THE KALISPELL AREA FISHED

5.3 Model structure

An unusual feature of the TER model is that it combines choice among sites with choice of trip length in one nest of the model. This implies that an angler sequentially chooses first whether to go to a river or lake, and then whether to take a long or short trip. However, in the MOR data, longer multi-day trips are more frequently associated with lake sites than river sites. Few anglers take long trips to river sites; they take these trips to lakes. One reason is that anglers have cabins at lakes more often than at river sites. In my view, the choice of trip length and lake vs. river site is simultaneous for most anglers, or sequential in the reverse order: anglers first decide what type of trip length (e.g. going on a week's vacation vs. going away for the day or overnight) and then where to go. Moreover, they are likely to consider different sets of sites depending on the length of trip that they have chosen -- many places near home would make no sense as a destination for a week's vacation, and many sites very distant would make no sense as a day trip, given the time needed to get there and back. This is not captured in the TER nesting structure.

The model structure also pays inadequate attention to the *type* of fishing -- what species of fish, and what method of fishing (e.g., fly-fishing vs. bait-fishing). This is introduced into the middle nest of the TER model, but nowhere else. Thus, if the target species is not trout, or if the trip is not fly-fishing, in the TER model this makes the angler more likely to choose a lake site versus a river site in the lake/river split. But, nothing in the model accounts for the choice of target species or the choice of fishing mode. Also, the model does not allow the choice of target species or fishing mode to influence the choice of a particular river site/lake site, or the choice of trip length. In the Southcentral Alaska study, I allowed the angler to choose a target species and this influenced the choice of site because different choice sets of sites were associated with different species. By contrast, the TER model assumes that the species and mode of fishing have no influence on which river site, or which lake site, an angler chooses.

Why does model structure matter? What difference does the nesting structure make? From a statistical point of view, it makes a considerable difference, for at least two reasons. First, when a nested logit model is estimated by sequential ML, as in the TER Report, changing the nesting structure changes both *which* variables are allowed to influence angler's choices and *how* they exert this influence. This, in turn, affects estimates of model coefficients. Second, regardless of whether one uses sequential ML or FIML, the nesting structure in a generalized logit model imposes very specific assumptions about the pattern of correlation among the random components in the underlying RUM. These have a powerful influence on predictions of behavior and estimates of value. Within each branch of a nest in the logit model, the items within the branch are required to satisfy a property known as Independence of Irrelevant Alternatives (IIA). For the tree as whole, the model structure implies a degree of correlation among the items within a branch, but not between items in one branch and items in another. If the assumed nesting structure is incorrect and the covariance structure among the stochastic components is mis-specified, this causes the coefficient estimates to be biased. Alvarez and Nagler (1994) have shown that the bias can be large, and can affect not just the absolute values of the coefficient estimates but also their ratios.

Although the TER research team model the choice of trip length in the bottom nest of their tree, they do so in a peculiar manner which is undermined by their approach to measuring travel cost. As I noted earlier, in order to reduce the estimates of lost consumer's surplus, they omit several components of travel cost that most other researchers would have included. Several of these omitted components are items that vary with trip length -- for example, costs of food and lodging, and on-site time. *Without these variables, there is no basis for modelling the choice of trip length as an economic decision.* There is no point in even collecting data on trip length, if one is going to exclude the economic variables that determine it. The TER research team sought to finesse this by including dummy variables for trip length in the equation to explain to explain trip length. Not surprisingly, these variables have highly significant coefficients. But, most people would consider regressing a variable on itself to be a dubious way of "explaining" it.

5.4 Model Estimation

Not only did the TER research team's decision to combine site choice with choice of trip length cause their model to be mis-specified, but it also created a nightmare for them in estimating the model because it expanded the number of choice items at the bottom of the tree fourfold, from 97 to 388. Because they could not cope with so many choices, they resorted to a technique of sampling a subset of the alternatives. The TER model implies that anglers visiting a river site choose among 236 (= 4x59) alternative site/length combinations, and those visiting a lake choose among 152 (= 4x38) alternative site/length combinations. However, when the TER research team actually estimated the model, they assumed that anglers visiting a river site choose among only 25 alternatives, and similarly for anglers visiting a lake.[18] This certainly reduced the dimensionality of the model and simplified its estimation. This approach was first suggested by Professor McFadden. It involves randomly sampling some fraction of the alternatives -- the TER research team settled on 25 in this case, amounting to 11% of the alternatives for rivers, and 16% for lakes.

McFadden (1978) shows that, if the items satisfy the IIA property, the estimates obtained through sampling of alternatives are consistent estimates of the true coefficients. However, I have three reservations about the use of sampling in this case. First, appealing to the asymptotic of consistency seems inappropriate where there are such tiny samples of trips -- it is more like wishful thinking. Second, the TER research team implemented the sampling incorrectly. Instead of sampling randomly without replacement, they sampled non-randomly and with replacement. They produced a set of 25 alternatives which involves duplications of the same choice items within the set. We believe that this was unintentional, and it may have resulted from carelessness or haste in performing the final model runs. Earlier model runs had used a different version of the model from what appears in the TER Report, in which there were 6 rather than 4 trip lengths, resulting in 6x97 rather than 4x97 alternatives. It seems that the sampling of 25 alternatives was

[18] This is mentioned nowhere in the report; it just shows up in the computer code.

performed at that stage of the analysis. When the change was made to 4 trip lengths, the alternatives selected for the sample were not appropriately modified -- the top three trip lengths were collapsed into the new top trip length. The result is both an excess proportion of the 25 alternatives in that length category and, sometimes, a duplication with two or more alternatives being the same site/length combination. I understand that Professor McFadden's result on the asymptotic reliability of coefficient estimates under sampling does not hold in these circumstances.

Moreover, Professor McFadden's result requires the multinomial logit structure with its IIA property. Thus, the IIA property is fundamental to both the specification of the TER model and the use of sampling to simplify its estimation. It is all the more surprising that the TER research team never got around to testing whether the data satisfy this property. My assistant Craig Mohn has tested for IIA among the river and lake choices at the bottom of the tree using the Hausman-McFadden (1984) test and found that it is uniformly rejected in both cases. McFadden (1984) notes that the test is not very powerful "unless deviations from the multinomial structure are substantial." That assuredly applies to the TER model.

The rejection of IIA implies that the model is mis-specified. This can be a symptom of many problems -- not only an incorrect nesting structure but also measurement error in variables that introduces some correlation among alternatives. Both are likely to be present in this case. Whatever the cause, the consequence of the failure of the IIA test is that one can have little confidence in the coefficient estimates presented in the TER report.

It is also evident that the coefficient estimates are fragile and non-robust. Table 12 illustrates this by comparing several of the coefficients in the river branch at the bottom of the tree when one uses the full set of 4*59 alternatives rather than the 25 alternatives employed by the TER research team. This is the estimation that they wanted to perform but could not because it was too large for them to handle. Observe how substantially the coefficients change. Other evidence of fragility is provided by the divergence between the FIML and sequential ML estimates of coefficients in the TER Report.

TABLE 12: INSTABILITY IN ESTIMATED COEFFICIENTS FOR CHOICE AMONG SITE/LENGTH ALTERNATIVES FOR RIVERS

	BIOMASS	AESTHETICS	TRAVEL COST	BIOMASS RATIO (1)	AESTHETICS RATIO (2)
TER REPORT (sample of alternatives)	0.1681	0.2693	-0.069	2.44	3.90
Full set of alternatives	0.29133	0.39385	-0.0844	3.45	4.67

NOTES: (1) Absolute value of ratio of coefficient on biomass to coefficient on travel cost
(2) Absolute value of ratio of coefficient on aesthetics to coefficient on travel cost

6. Estimate of Recreation Losses

6.1 Fishing Recreation Loss

The TER Report assumes away the loss of consumer's surplus for nonresidents who fish in Montana. There is no justification for this. In my view, it is clearly an important and large component of the impact of hazardous releases on fishing in the Upper Clark Fork Basin. At this point, however, I have not had time to form an estimate of it. I focus instead on the loss to fishing by resident anglers.

The TER Report presents estimates of both the annual damage to resident anglers and the discounted total loss over the period 1981 - 2000. Here, I focus on the estimate of annual damage. The TER Report presents three estimates.[19] The base estimate is $104,000 per year, based on a particular scenario for the effects of the release of hazardous substances on trout biomass in the Upper Clark Fork River Basin and using the sequential ML model coefficients. At this time, I am not in a position to form an opinion regarding the effects of hazardous releases on the aquatic ecosystem in the Basin. For the sake of analysis in this section, I take the TER assessment of these effects and examine what monetary value should be placed on them. Moreover, I have not yet had the time to replicate the calculation of lost consumer's surplus. For now, I assume that the baseline estimate was calculated correctly, given the scenario TER chose to employ. The TER report applies two cumulative adjustments to this baseline, as follows:

TER ADJUSTMENT	ADJUSTMENT FACTOR	ANNUAL DAMAGES
Measurement error in biomass	x 1.19	$130,500
Upper bound on 95% confidence interval	x 2.07	$411,500

The adjustment for error in measuring biomass is based on the Monte Carlo simulation that was discussed above. The second adjustment is intended as a "conservative upper-bound" on damages. However, it is hardly conservative, because it is based on estimate of variance that ignores the imprecision introduced by the use of sample weights. Also, it is not clear from the TER Report whether the standard errors of the sequential ML coefficient estimates have been adjusted upwards to correct for the fact that the estimation of upper levels of the model involve estimated explanatory variables.

[19] It also presents an estimate based on the FIML model. I discount this because the wrong sign of coefficients in that model highlights the havoc that mis-specification can wreak.

I now modify these adjustments to take account of the conclusions I have reached about the surveys, the data, and the model structure and estimation that are used in TER's analysis. I will not consider a 95% confidence interval upper bound, since I have not yet had time to investigate this.

If one adopts TER's approach to modelling recreation choices by resident anglers but merely corrects for the gross errors in their analysis, there are four items that must be corrected: (i) the undercount of fishing trips, (ii) the bias introduced by not estimating the river model with the full set of site/length alternatives that the TER research team postulates, (iii) the errors in measuring travel cost, and (iv) the error in measuring the biomass variable. If one assumed that the release of hazardous substances in the Basin degraded sites from a value of 1 in the AESTHETICS variable to a value of 0, this would add another component to the estimate of loss. Setting that aside, the damage estimate in the TER Report should be adjusted as follows:

RECOMMENDED ADJUSTMENT	ADJUSTMENT FACTOR	ANNUAL DAMAGES
Raw data error: undercount of trips	x 2	$208,000
Estimation error from not using full set of river alternatives	x 1.41	$293,280
Measurement error, travel cost	x 2	$586,500
Measurement error, biomass	x 2.4	$1,407,700

6.2 Non-Fishing Recreation Loss

One of the oddest features of the TER study is that, while the research team went to great lengths to collect data on a wide variety of outdoor recreation activities by Montana residents, including several that are likely to have been impacted by the release of hazardous substances in the Upper Clark Fork Basin, it then ignored all activities other than fishing. A by-product of doing this was that the TER Report fails to present any estimate of recreation damages for activities other than fishing. There can be no good justification for this failure. I have not yet had time to form an estimate of the possible damage to non-fishing recreation in Montana by either residents or non-residents. However, it would clearly be significant.

BIBLIOGRAPHIC REFERENCES

R. Michael Alvarez and Jonathan Nagler, "Correlated Disturbance in Discrete Choice Models: A Comparison of Multinomial Probit Models and Logit Models," Pasadena: California Institute of Technology, Division of the Humanities and Social Sciences, Social Science Working Paper 914, 1994.

Barbara A. Bailar, "Information Needs, Surveys and Measurement Errors" in Daniel Kasprzyk et al., (eds) *Panel Surveys* New York: John Wiley, 1989

R. J. Carroll, D. Ruppert and L. A. Stefanski, *Measurement Error in Nonlinear Models* London: Chapman & Hall, 1995.

Raymond J. Carroll, "Covariance Analysis in Generalized Linear Measurement Error Models," *Statistics in Medicine* 8 (1989), 1075-1093.

Frank J. Cesario, "Value of Time in Recreation Benefit Studies," *Land Economics* 52 (February 1976), 32 -41.

Marion Clawson, *Methods of Measuring the Demand for and Value of Outdoor Recreation* Washington, DC: Resources for the Future, Inc., Reprint Number 10, February 1959.

William H. Desvousges et al., *Techniques to Measure Damages to Natural resources: Final Report.* Prepared for CERCLA 301 Task Force, US Department of the Interior, EPA Contract Number 68-01-7033, Research Triangle Institute Project Number 3142-05 FR, September 1986.

Greg J. Duncan, F. Thomas Juster, and James N. Morgan, "The Role of Panel Studies in a World of Scarce resources," in R. W. Pearson and R. F. Boruch (eds), *Survey Research Designs: Towards a Better Understanding of their Costs and Benefits* Berlin: Springer-Verlag, Lecture Notes in Statistics, Volume 38, 1986.

Wayne A. Fuller, *Measurement Error Models* New York: John Wiley, 1987.

Hausman, J.A., Leonard, G.K. and McFadden, D. (1993) Assessing use value losses Caused by Natural Resource Injury. In: Hausman, J.A. (ed.) *Contingent Valuation: A Critical Assessment.* North-Holland, New York, pp. 341-368.

Jerry Hausman and D. McFadden, "A Specification Test for the Multinomial Logit Model," *Econometrica* 52 (September 1984), 1219-41.

Hotelling, Harold (1949) letter contained in U.S. National Park Service, *The Economics of Public Recreation: An Economic Study of the Monetary Evaluation of Recreation in the National Parks.* Washington, DC.

Loftus, E., Klinger, M.R., Smith, K.D. and Fiedler, J. (1990) A Tale of Two Questions: Benefits of Asking More than One Question. *Public Opinion Quarterly* 54, 330-345.

Daniel McFadden, "Econometric Analysis of Qualitative Response Models," in Z. Griliches and M. Intriligator (eds.) *Handbook of Econometrics, Vol II,* Amsterdam: Elsevier, 1984, 1396-1456.

Daniel McFadden, "Modelling the Choice of Residential Location," in A. Karlquist, L. Lundquist, F. Snickars, and J. Weibull, *Spatial Interaction Theory and Planning Models* Amsterdam: North-Holland, 1978, 75-96.

Robert C. McFarland and Janet E. Hughes, *Montana Statewide Angling Pressure Mail Survey* Montana Department of Fish, Wildlife and Parks, April 1994

Neter, J. and Waksberg, J. (1964) A Study of Response Errors in Expenditures Data from Household Interviews. *Journal of the American Statistical Association* 3, 18-55.

B. Rosner, W. C. Willett and D. Spiegelman, "Correction of Logistic Relative Risk Estimates and Confidence Intervals for Systematic Within-Person Measurement Error," *Statistics in Medicine* 8 (1980), 1051-1069

Daniel W. Schafer, "Covariate Measurement Error in General Linear Models," *Biometrika* 74 (1987), 385-91.

V. Kerry Smith, William H. Desvousges, and Mathew P. McGivney, "The Opportunity Cost of Travel Time in Recreation Demand Models," *Land Economics* Vol.59, No.3, (August 1983) 259 -78.

Seymour Sudman and Robert Ferber, *Consumer Panels* Chicago: American Marketing Association, 1979

Andrew H. Trice and Samuel E. Wood, "Measurement of Recreation Benefits," *Land Economics* Vol. 34, No. 3 (August 1958), 195 - 207

U.S. Department of the Interior, Fish and Wildlife Service and U.S. Department of Commerce, Bureau of the Census. *1991 National Survey of Fishing, Hunting and Wildlife-Associated Recreation: Montana* U.S. Government Printing Office, Washington, DC 1993.

Alice S. Whittemore and Joseph B. Keller, "Approximations for Regression with Covariate Measurement Error,' *Journal of the American Statistical Association*, 83 (December 1988), 1057-66.

APPENDIX

Cases in which I have testified or been deposed since 1991:

Charles W. Brydon et al. v. East Bay Municipal Utility District, California Superior Court for Contra Costa County, Case No. C91-03159, Declaration on behalf of EBMUD August 18, 1992.

California State Water Resources Control Board, Interim Water Rights Hearing, Rebuttal testimony on behalf of the Natural Heritage Institute, August 1992.

Chaparral Greens vs. Baldwin Builders, California Superior Court for San Diego County, Case No. 671181, Declaration on behalf of Chaparral Greens, September, 1994.

U.S. v Montrose. Co-authored expert report for NOAA on assessment of interim lost use values associated with PCB and DDT contamination in the Southern California Bight, October 1994.

American Rivers, Inc. v. Crown Butte Mines, Inc. Mineral Patent Application No. MTM-080972 (US Department of Interior), Affidavit submitted on behalf of American Rivers, January 1995.

RESUME IS ATTACHED

September 1995

CURRICULUM VITAE

Name: W. Michael Hanemann

Title: Professor of Agricultural and Resource Economics

Address: Department of Agricultural and Resource Economics
University of California
Berkeley, California 94720
(510) 642-2670

Birthdate and Place: August 26, 1944, Manchester, England

Citizenship: U. S. citizen

Education

Institution	Degree	Year	Field
Oxford University, England	B.A.	1965	Philosophy, Politics, and Economics
London School of Economics, England	M.Sc.	1967	Development Economics
Harvard University	M.A.	1973	Public Finance and Decision Theory
Harvard University	Ph.D.	1978	Economics

Dissertation title: "A Theoretical and Empirical Study of the Recreation Benefits from Improving Water Quality in the Boston Area," Harvard University, 1978, 532p.

Employment

1967-68: Assistant to the Director, Unit for Economic and Statistical Studies on Higher Education, London School of Economics.

1970-1975: Teaching Fellow, Department of Economics, Harvard University.

1970-1975: Staff Economist/Consultant, Urban Systems Research & Engineering, Inc., Cambridge, Massachusetts.

1976: Lecturer, Department of Economics, Northeastern University, Boston, Massachusetts.

1976-1978: Acting Assistant Professor, Department of Agricultural and Resource Economics, University of California at Berkeley.

1978-1984: Assistant Professor, Department of Agricultural and Resource Economics, University of California at Berkeley.

1984-1995: Associate Professor, Department of Agricultural and Resource Economics, University of California at Berkeley.

1995- : Professor, Department of Agricultural and Resource Economics, University of California at Berkeley.

Other Professional Activities

Member, UNEP Working Group on Benefits of Biodiversity Conservation (1992-93)

University Fellow, Resources for the Future (1989-90, 1990-91, 1991-92).

Consultant to California State Water Resources Control Board (1986-89); California Attorney General's Office (1987-); Alaska Attorney General's Office (1989-); NOAA (1991-); The Mayor of Los Angeles Blue Ribbon Committee on Water Rates (1992).

Member (1989-90) and Chair (1991) Association of Environmental and Resource Economists Committee on Publication of Enduring Quality.

Member (1990-91) American Association of Agricultural Economics Committee on Quality of Research Discovery.

Invited as Bank of Norway Leif Johansen Visiting Lecturer at the University of Oslo, Department of Economics, September, 1987.

Member of the National Academy of Sciences/National Research Council Committee to Review the Glen Canyon Environmental Studies Program (1986-91).

Reviewer for the National Science Foundation, National Acid Precipitation Assessment Program, *American Economic Review*, *American Journal of Agricultural Economics*, *Econometrica*, *International Economic Review*, *Journal of Environmental Economics and Management*, *Land Economics*, *Natural Resource Modeling*.

Awards

Winner, American Agricultural Economics Association Outstanding Journal Article Award, 1990.

Winner, Western Agricultural Economics Association Outstanding Journal Article Award, 1990.

Supervised Winner of Resources for the Future's Prize for Best Ph.D. Dissertation in Natural Resource Economics, 1989.

Publications

Books and Monographs

Lake, Elizabeth; Hanemann, W. Michael; and Oster, Sharon. *Who Pays for Clean Water? The Distribution of Water Pollution Control Costs.* Boulder, Colorado: Westview Press, 1979.

Bockstael, Nancy E.; Hanemann, W. Michael; and Strand, Ivar. *Measuring the Benefits of Water Quality Improvements Using Recreation Demand Models.* Washington, D. C.: U. S. Environmental Protection Agency, October, 1984.

Wegge, Thomas C.; Hanemann, W. Michael; and Strand, Ivar E. *An Economic Assessment of Marine Recreational Fishing in Southern California.* U. S. NOAA, National Marine Fisheries Service Technical Memorandum, NMFS-SWR-015, 1986.

Fisher, Anthony; Hanemann, Michael; Harte, John; Horne, Alexander; Ellis, Gregory; and Von Hippel, David. *Economic Valuation of Aquatic Ecosystems.* Prepared for the U. S. Environmental Protection Agency. Berkeley: University of California, October, 1986.

Hanemann, Michael; Lichtenberg, Erik; Zilberman, David; Chapman, David; Dixon, Lloyd; Ellis, Greg; and Hukkinen, Janne. "Economic Implications of Regulating Agricultural Drainage to the San Joaquin River." *Regulation of Agricultural Drainage to the San Joaquin River.* SWRCB Order No. W.Q. 85-1, Technical Committee Report. Appendix G (two vols.). Sacramento, California: State Water Resources Control Board, August, 1987, 443p.

Strong, David; Hanemann, Michael; and Wallace, L. Tim. "Private and Public Sector Economic Impacts Resulting from Regulation of Agricultural Drainage to the San Joaquin River." *Regulation of Agricultural Drainage to the San Joaquin River.* SWRCB Order No. W.Q. 85-1, Technical Committee Report. Appendix J, Indirect and Induced Economic Impacts. Sacramento, California: State Water Resources Control Board, August, 1987, 207p.

Committee to Review the Glen Canyon Environmental Studies. *The Canyons of the Colorado: A Review of the Glen Canyon Environmental Studies.* Washington, D.C.: National Academy Press, 1987. 203p.

Hanemann, W. Michael; Wegge, Thomas C.; and Strand, Ivar E. *Development and Application of a Predictive Model to Analyze the Economic Effects of Species Availability.* U.S. NOAA National Marine Fisheries Service Administrative Report SWR-89-02, June, 1989.

Carson, Richard T.; Mitchell, Robert C.; Hanemann, W. Michael; Kopp, Raymond J.; Presser, Stanley, and Ruud, Paul A. *A Contingent Valuation Study of Lost Passive Use Values Resulting from the Exxon Valdez Oil Spill.* Washington, D.C.: Resources for the Future, November, 1992.

Carson, Richard T.; W. Michael Hanemann; Raymond J. Kopp; Jon A. Krosnick; Robert C. Mitchell; Stanley Presser; Paul A. Ruud; V. Kerry Smith. *Prospective Interim Lost Use Value Due to DDT and PCB Contamination in the Southern California Bight.* La Jolla, California: Natural Resource Damage Assesment, Inc., September 30, 1994.

Contributions to Books

Erlenkotter, Donald; Hanemann, W. Michael; Howitt, Richard E.; and Vaux, Henry J., Jr. "The Economics of Water Development and Use." In *California Water Planning and Policy: Selected Issues*, edited by Ernest Engelbert. Davis: University of California, Water Resources Center, June, 1979.

Hanemann, W. Michael. "Quality and Demand Analysis." In *New Directions in Econometric Modeling and Forecasting in U. S. Agriculture*, edited by Gordon C. Rausser. New York: Elsevier/North-Holland, Inc., 1982, pp. 55-98.

Fisher, Anthony C.; and Hanemann, W. Michael. "Endangered Species: The Economics of Irreversible Damage." In *Economics of Ecosystem Management*, edited by D. O. Hall, N. Myers, and N. S. Margaris. Dordrecht: Dr. W. Junk Publishers, 1985.

Fisher, Anthony C.; and Hanemann, W. Michael. "Option Value and the Extinction of Species." In *Advances in Applied Micro-Economics, Volume 4*, edited by V. Kerry Smith. Greenwich, Connecticut: JAI Press, 1986, pp. 169-190.

Hanemann, W. Michael. "WTP and WTA as Benefit Measures—How Do They Differ? How Can They Be Measured?" In *Proceedings of a Workshop on Economic Non-Market Evaluation of Losses to Fish, Wildlife and Other Environmental Resources*, edited by Philip A. Meyer and Stephen O. Anderson. Sausalito, California: Bay Institute of San Francisco, May, 1987, pp. 20-42.

______. "Economics and the Preservation of Biodiversity." In *Biodiversity*, edited by E. O. Wilson. Washington, D. C.: National Academy Press, 1988.

______. "Commentary on 'Valuation of Genetic Resources.'" In *The Preservation and Valuation of Biological Resources*, edited by Gordon H. Orians, Gardner M. Brown Jr., William E. Kunin and Joseph E. Swierzbinski. Seattle, Washington: University of Washington, 1990, pp. 233-239.

Loomis, John B.; Wegge, Thomas; Hanemann, Michael; and Kanninen, Barbara. "The Economic Value of Water to Wildlife and Fisheries in the San Joaquin Valley: Results of a Simulated Voter Referendum." In *Transactions of the 55th North American Wildlife and Natural Resources Conference*, Wildlife Management Institute, Washington, D. C., 1990, pp. 259-268.

______. "Reviewer's Comments" In David Imber, Gay Stevenson and Leanne Wilks, *A Contingent Valuation Survey of the Kakadu Conservation Zone.* Canberra, Australia: Resource Assessment Commission, RAC Research Paper No. 3, February, 1991, pp. 187-192.

Loomis, John B.; Hanemann, Michael; Kanninen, Barbara; and Wegge, Thomas. "Willingness to Pay to Protect Wetlands and Reduce Wildlife Contamination from Agricultural Drainage." In *The Economics and Management of Water and Drainage in Agriculture*, edited by Ariel Dinar and David Zilberman. Boston: Kluwer Academic Publishers, 1991, pp. 411-429.

Hanemann, W. Michael. "Natural Resource Damages for Oil Spills in California." In Kevin M. Ward and John W. Duffield, *Natural Resource Damages: Law and Economics*, New York: John Wiley & Sons, Inc., 1992, pp. 555-580.

______. "Preface: Notes on the History of Environmental Valuation in the USA." In Stale Navrud (ed), *Pricing the Environment: The European Experience*, Oxford University Press, 1992.

Kanninen Barbara J.; Chapman, David J.; and Hanemann, W. Michael. "Survey Data Collection: Detecting and Correcting for Biases in responses to Mail and Telephone Surveys." in US Department of Commerce, Bureau of the Census, *Proceedings 1992 Annual Research Conference* 1992, pp. 507-522.

Fisher, Anthony C.; and Hanemann, W. Michael. "Valuation of Tropical Forests." In Karl-Goran Maler and Partha Dasgupta (eds) *The Environment and Emerging Development Issues*, Oxford University Press (forthcoming).

______. "Assessing Climate Change Risks: Valuation of Effects." In Joel Darmstadter (ed) *Assessing Climate Change Risks*, Washington, DC: Resources for the Future, 1993.

Hanemann, W. Michael; and Kriström, Bengt. "Preference Uncertainty, Optimal Designs and Spikes." In Per-Olov Johansson (ed) *Advances in Environmental Economics*, England: Manchester University Press, 1994.

Articles

Chugh, Lal C.; Hanemann, W. Michael; and Mahapatra, S. "Impact of Pollution Control Regulations on the Market Risk of Securities in the U. S." *Journal of Economic Studies*, Vol. 5 (May, 1978), pp. 64-70.

Hanemann, W. Michael. "Measuring the Worth of Natural Resource Facilities." *Land Economics*, Vol. 56 (October, 1980), pp. 482-490.

______. "Marginal Welfare Measures for Discrete Choice Models." *Economics Letters*, Vol. 13 (1983), pp. 129-136.

______. "Entropy as a Measure of Consensus in the Evaluation of Recreation Site Quality." *Journal of Environmental Management*, Vol. 18 (April, 1984), pp. 241-251.

______. "Discrete/Continuous Models of Consumer Demand." *Econometrica*, Vol. 52 (May, 1984), pp. 541-561.

______. "Welfare Evaluations in Contingent Valuation Experiments with Discrete Responses." *American Journal of Agricultural Economics*, Vol. 66 (August, 1984), pp. 332-341.

______. "Some Issues in Continuous- and Discrete-Response Contingent Valuation Studies." *Northeastern Journal of Agricultural Economics*, Vol. 14 (April, 1985), pp. 5-13.

Fisher, A. C.; and Hanemann, W. M. "Environmental Damages and Option Values." *Natural Resources Modeling*, Vol. 1 (Fall, 1986), pp. 111-124.

Fisher, Anthony C.; and Hanemann, W. Michael. "Quasi-Option Value: Some Misconceptions Dispelled." *Journal of Environmental Economics and Management*, Vol. 14 (1987), pp. 183-190.

Hanemann, W. Michael. "Welfare Evaluations in Contingent Valuation Experiments with Discrete Responses: Reply." *American Journal of Agricultural Economics*, Vol. 69 (February, 1987), pp. 185 and 186.

Bockstael, Nancy E.; Hanemann, W. Michael; and Kling, Catherine L. "Estimating the Value of Water Quality Improvements in a Recreational Demand Framework." *Water Resources Research*, Vol. 23 (May, 1987), pp. 951-960.

Bockstael, Nancy E.; Strand, Ivar E.; and Hanemann, W. Michael. "Time and the Recreational Demand Model." *American Journal of Agricultural Economics*, Vol. 69 (May, 1987), pp. 293-302.

Hanemann, W. Michael. "Information and the Concept of Option Value." *Journal of Environmental Economics and Management*, 16:1 (January, 1989), pp. 23-37.

La France, Jeffrey T.; and Hanemann, W. Michael. "The Dual Structure of Incomplete Demand Systems." *American Journal of Agricultural Economics*, Vol. 71 (May, 1989), pp. 262-274.

Hanemann, W. Michael. "Welfare Evaluations in Contingent Valuation Experiments with Discrete Response Data: Reply." *American Journal of Agricultural Economics*, Vol. 71 (November, 1989), pp. 1057-1061.

Carson, Richard T.; Hanemann, Michael; and Steinberg, Dan. "A Discrete Choice Contingent Valuation Estimate of the Value of Kenai King Salmon." *Journal of Behavioral Economics*, Vol. 19, Number 1 (1990), pp. 53-68.

Fisher, Anthony C.; and Hanemann, W. Michael. "Information and the Dynamics of Environmental Protection: The Concept of the Critical Period." *Scandinavian Journal of Economics*, Vol. 92, Number 3 (1990), pp. 399-414.

Fisher, Anthony C.; and Hanemann, W. M. "Option Value: Theory and Measurement." *European Review of Agricultural Economics* Vol. 17 No. 2, 1990, pp. 167-180.

Hanemann, W. Michael. "Willingness to Pay and Willingness to Accept: How Much Can They Differ?" *American Economic Review*, 81:3, June 1991, pp. 635-647; reprinted in The Economics of the Environmen, ed. by W.E. Oates, Cambridge, U.K.: University Press, 1994, pp. 471- 483.

Fisher, Anthony C.; Hanemann, W. Michael; and Keeler, Andy. "Integrating Fishery and Water Resource Management: A Biological Model of a California Salmon Fishery." *Journal of*

Environmental Economics and Management 20, 1991, pp. 234-261

Hanemann, Michael; Loomis, John; and Kanninen, Barbara. "Statistical Efficiency of Double-Bounded Dichotomous Choice Contingent Valuation." *American Journal of Agricultural Economics* 73, November. 1991. 1255-1263.

Hanemann, Michael; and Morey, Edward. "Separability, Partial Demand Systems and Consumer's Surplus Measures." *Journal of Environmental Economics and Management* 22:3, 1992, pp. 241-258.

Hanemann, W. Michael; Johansson, Per-Olov; Kriström, Bengt; and Mattson, Leif. "Natural Resource Damages for Chernobyl." *Environment & Resource Economics* 2, 1992, pp. 523-525.

Hanemann, W. Michael; Strand, Ivar E. "Natural Resource Damage Assessment: Economic Implications for Fisheries Management." *American Journal of Agricultural Economics* 75, December, 1993.

Hanemann, W. Michael, "Designing New Water Rates for Los Angeles" in *Water Resources Update*, The Universities Council on Water Resources, Issue No. 92, Carbondale, IL, Summer, 1993.

Hanemann, Michael, *The Economics of Biodiversity*, "Incentives for Protecting North American Biodiversity," vol. 1, No. 3, Summer 1994, pp. 24-25.

Hewitt, Julie A.; and Hanemann, W. Michael. "A Discrete/Continuous Choice Model of Demand Under Block Rate Pricing." *Land Economics*, 71:2, May, 1995, pp. 173-192.

Papers Submitted for Publication

Hanemann, Michael; Lichtenberg, Erik; and Zilberman, David. "Conservation versus Cleanup in Agricultural Drainage Control." Department of Agricultural and Resource Economics, University of Maryland, September, 1989.

Dale, Larry; and Hanemann, W. M. "Transactions Costs and the Metering of Urban Water Use in California." Department of Agricultural and Resource Economics, University of California. Berkeley, June, 1990.

Carson, Richard; Flores, Nicholas E; and Hanemann, W. Michael. "On The Creation and Destruction of Public Goods: The Matter of Sequencing." Department of Economics, UC San Diego. April 1992.

Hanemann, W. Michael. "Consumer Demand with Multiple Linear Constraints: A Global Analysis." Department of Agricultural and Resource Economics, University of California. Berkeley, March, 1993.

Carson, Richard T.; Mitchell, Robert C.; Hanemann, W. Michael; Kopp, Raymond J.; Presser, Stanley; and Ruud, Paul A. *Contingent Valuation and Lost Passive Use: Damages from the Exxon Valdez*. Resources for the Future, Washington, D.C., Discussion paper 95-02, January 1995.

Carson, Richard T.; Hanemann, W. Michael; Kopp, Raymond J.; Krosnick, Jon A.; Mitchell, Robert C.; Presser, Stanley; Ruud, Paul A.; and Smith, V. Kerry. *Temporal Reliability of Estimates from Contingent Valuation*. Resources for the Future, Washington, D.C., Discussion paper 95-37, August 1995.

Working Papers

Hanemann, W. Michael. "The Value of Lifesaving Reconsidered." Department of Agricultural and Resource Economics, Working Paper No. 113, University of California. Berkeley, August, 1979.

Hanemann, W. Michael; and Farnsworth, Richard L. "Risk Preferences vs. Perceptions in the Adoption of IPM." Department of Agricultural and Resource Economics, Working Paper No. 81, University of California. Berkeley, March, 1980.

Hanemann, W. Michael. "Quality Changes, Consumer's Surplus, and Hedonic Price Indices." Department of Agricultural and Resource Economics, Working Paper No. 116, University of California. Berkeley, November, 1980.

______. "Water Quality and Recreation Demand." Department of Agricultural and Resource Economics, Working Paper No. 164, University of California. Berkeley, March, 1981.

Hanemann, W. Michael; and Tsur, Yacov. "Econometric Models of Discrete/Continuous Supply Decisions Under Uncertainty." Department of Agricultural and Resource Economics, Working Paper No. 195, University of California. Berkeley, March, 1982.

Hanemann, W. Michael. "Applied Welfare Analysis with Qualitative Response Models." Department of Agricultural and Resource Economics, Working Paper No. 241, University of California. Berkeley, October, 1982.

______. "Some Further Results on Exact Consumer's Surplus." Department of Agricultural and Resource Economics, Working Paper No. 190, University of California. Berkeley, December, 1982.

______. "Econometric Models of the Demand for Quality-Differentiated Goods." Department of Agricultural and Resource Economics, Working Paper No. 272, University of California. Berkeley, July, 1983.

______. "A Critique of 'Identifying Structural Equations Through the Nonlinearity of a Single Price Gradient' and 'Identifying Structural Equations with Single Market Data' by Rob Mendelsohn." Department of Agricultural and Resource Economics, Working Paper No. 300, University of California. Berkeley, June, 1984.

Hanemann, W. Michael; Tsur, Yacov; and Zilberman, David. "Pesticide Usage and the Choice of Pest Control: A Switching Regression Analysis." Department of Agricultural and Resource Economics, Working Paper No. 301, University of California. Berkeley, June, 1984.

Hanemann, W. Michael. "On Reconciling Different Concepts of Option Value." Department of Agricultural and Resource Economics, Working Paper No. 295, University of California.

Berkeley, July, 1984.

La France, Jeffrey T.; and Hanemann, W. Michael. "The Integration of Some Common Demand Systems." Department of Agricultural and Resource Economics, Working Paper No. 320, University of California. Berkeley, July, 1984.

Hanemann, W. Michael. "Introducing Site Quality Into Recreation Demand Functions." Department of Agricultural and Resource Economics, Working Paper No. 338, University of California. Berkeley, November, 1984.

______. "Multiple Site Demand Models. Part I: Some Preliminary Considerations." Department of Agricultural and Resource Economics, Working Paper No. 336, University of California. Berkeley, November, 1984.

______. "Multiple Site Demand Models. Part II: Review of Existing Models and Development of New Models." Department of Agricultural and Resource Economics, Working Paper No. 337, University of California. Berkeley, November, 1984.

______. "Welfare Analysis with Discrete Choice Models." Department of Agricultural and Resource Economics, University of California. Berkeley, August, 1985.

Carson, Richard T.; Hanemann, W. Michael; and Mitchell, Robert Cameron. "The Use of Simulated Political Markets to Value Public Goods." Economics Department, University of California. San Diego, October, 1986.

Hanemann, W. Michael. "Institutional Problems of Production Agriculture Related to the Regulatory Process." Department of Agricultural and Resource Economics, University of California. Berkeley, March, 1987.

Hanemann, W. Michael, "Three Approaches to Defining 'Existence' or 'Non-Use' Value Under Certainty. Department of Agricultural and Resource Economics, Working Paper No. 691, University of California, Berkeley. July, 1988.

Hanemann, Michael; Lichtenberg, Erik; and Zilberman, David. "The Role of Water Conservation versus Cleanup in Agricultural Drainage Control." Department of Agricultural and Resource Economics, University of Maryland, July, 1989.

Keeler, A.; Fisher, A.; and Hanemann, W. M. "The San Francisco Bay/Delta Chinook Salmon Fishery: A Review and Analysis of Recent Trends." Department of Agricultural and Resource Economics, Working Paper No. 501, University of California. Berkeley, 1989.

Loomis, J.; Hoehn, J.; and Hanemann, Michael. "Testing the Fallacy of Independent Valuation and Summation in Multi-Part Policies: An Empirical Test of Whether 'Too Many Proposals Pass the Benefit-Cost Test'." Department of Environmental Studies, University of California, Davis, August, 1990.

Carson, Richard T.; Flores, Nicholas; Hanemann, W. Michael. "On the Nature of Compensable Value in a Natural Resource Damage Assessment" Natural Resource Damage Assessment, Inc.: La Jolla, California. December, 1991.

Carson, Richard T.; Conaway, M.B.; Hanemann, W.M.; Kopp, R.J.; Martin, K.; Mitchell, R.C.; Presser, S.; Ruud, P.A. "Valuing Winter Visibility Improvement in the Grand Canyon, Natural Resource Damage Assessment, Inc.: La Jolla, California, 1992.

Kriström, Bengt; Nyquist, Hans; and Hanemann, W. Michael. "Dichotomous Choice Contingent Valuation Experiment: A Mixed Distribution Approach." Natural Resource Damage Assessment, Inc.: La Jolla, California, 1993.

Hanemann, W. Michael. "Projected M&I Demand in the LADWP Service Area 1990-2010." Department of Agricultural and Resource Economics, Working Paper No. 644, University of California, Berkeley. February, 1993.

Albers, Heidi J.; Fisher, Anthony C.; and Hanemann, W. Michael. "Valuation and Management of Tropical Forests: A Theoretical and Empirical Analysis." Department of Agricultural and Resource Economics, Working Paper No. 681, University of California, Berkeley. July, 1993.

Chesnutt, Thomas W.; Bamezai, Anil; and Hanemann, W. Michael. "Revenue Instability Induced by Conservation Rate Structures: An Empirical Investigation of Coping Strategies." Department of Agricultural and Resource Economics, Working Paper No. 695, University of California, Berkeley. November, 1993.

Hanemann, W. Michael. "Contingent Valuation and Economics." Department of Agricultural and Resource Economics, Working Paper No. 697, University of California. Berkeley, 1994.

Hanemann, W. Michael. "Marginal Cost Pricing and the New LADWP Water Rates." Department of Agricultural and Resource Economics, Working Paper No. 682, University of California. Berkeley, 1994.

Research Reports

Hanemann, W. Michael; and Binkley, C. *The Recreation Benefits of Water Quality Improvements: Analysis of Day Trips in an Urban Setting*. EPA-600/5-78-010. Report submitted to the U. S. Environmental Protection Agency by Urban Systems Research and Engineering, Inc. Washington, D. C.: U. S. Environmental Protection Agency, 1978.

King, Diana; Hanemann, W. Michael; and Nimmons, John. *The Role of Gas and Electric Utilities in Direct Applications of Geothermal Resources*. Earl Warren Legal Institute, University of California. Berkeley, March, 1980.

Hanemann, W. Michael (contributor). *Market Shares Estimation Task Force Report: Projections for Hydrothermal Direct-Heat Systems Market Shares*. U. S. Department of Energy, December, 1980.

Hanemann, W. Michael. *Welfare Evaluations with Simulated Hypothetical Market Data: Bishop and Heberlein Revisited*. 1983.

Fairfax, Sally K.; and Hanemann, W. Michael, eds. *Emerging Research Needs and Priorities Concerning Minerals Development on National Forest Lands.* Department of Agricultural and Resource Economics, University of California. Berkeley, March, 1983.

Stern, Martin W.; Hanemann, W. Michael; and Eckhouse, Kathy. *A Case Study of Energy and Process Substitution for Frozen Broccoli: Geothermal Energy and the Retortable Pouch.* Department of Agricultural and Resource Economics, University of California. Berkeley, August, 1983.

Hanemann, Michael; Strand, Ivar; and Wegge, Thomas. *Albacore Sportfishing Economic Study.* Prepared for the National Marine Fisheries Service. Sacramento, California: Jones and Stokes Associates, Inc., May, 1986.

Hanemann, W. Michael. *Economic Valuation of Changes in the Catch of Sacramento River Chinook Salmon.* Department of Agricultural and Resource Economics, University of California. Berkeley, October, 1986.

Hall, C. H.; Lichtenberg, E. R.; Patton, S. E.; Layton, D. W.; Hanemann, W. M.; and Stuermer, D. H. *Costs of Alternative Treatment Technologies for 1,2-Dibromo-3-Chloropropane (DBCP) in California Groundwater Supplies.* Livermore, California: Lawrence Livermore National Laboratory, May, 1987.

Carson, Richard T.; Hanemann, W. Michael; and Wegge, Thomas C. *Final Report: Juneau Area Sport Fishing Economic Study.* Prepared for the Alaska Department of Fish and Game. Jones and Stokes Associates, Sacramento, California, December, 1987.

______. *Final Report: Southcentral Alaska Sport Fishing Economic Study.* Prepared for the Alaska Department of Fish and Game. Jones & Stokes Associates, Sacramento, California, December, 1987.

Hanemann, W. M.; and Dale, L. *Reasonable Municipal and Industrial Use in 2010 by SWP Contractors in Southern California* and *Reasonable Municipal and Industrial Use in 2010 in the Central Coast and San Francisco Bay HSAs.* Consultant's report submitted to the State Water Resources Control Board, Berkeley, 1988.

Dale, L.; and Hanemann, W. M. *The Cost of Re-Operating New Don Pedro Reservoir to Increase Spring Flow to the San Joaquin River.* Department of Agricultural and Resource Economics, University of California. Berkeley, June, 1990.

Dale, L.; and Hanemann, W. M. *Sizing of A Groundwater Bank.* Department of Agricultural and Resource Economics, University of California. Berkeley, June, 1990.

Jones & Stokes Associates, Inc. *Environmental Benefits Study of San Joaquin Valley's Fish and Wildlife Resources.* (JSA 87-150) Sacramento, California. Prepared by J. B. Loomis, W. M. Hanemann, and T. C. Wegge, September, 1990.

Carson, Richard T.; Hanemann, W. Michael; Kopp, Raymond. *A Nested Logit Model of Recreational Fishing Demand in Alaska*, June, 1989.

Jones & Stokes Associates, Inc. *Southeast Alaska Sport Fishing Economic Study* Final Research Report (JSA 88-028) Sacramento, California. December 1991.

Carson, Richard T.; Conaway, Michael B.; Hanemann W. Michael; Kopp, R.J.; Mitchell, R.C.; Presser, S.; Ruud, P.A. *Comments on the Benefit Analysis in the U.S. Environmental Protection Agency's Proposed Navajo Generating Station BART Action Decision Focus*, 1991.

Neal MacDougall, Hanemann, W. Michael, and Zilberman, David. *The Economics of Agricultural Drainage* Report submitted to the Central Valley Regional Water Quality Control Board, Standard Agreement No. 0-132-150-0. June 1992.

Hanemann, W. Michael. *Comment submitted to the Office of Environmental Affairs, Department of Interior, in response to the Notice of Proposed Rulemaking (58 FR 39328) Concerning Natural Resource Damage assessments*, 1993.

Dumas, Christopher F.; and Hanemann, W. Michael. *Economic Benefits of the United States Environmental Protection Agency's Proposed Bay-Delta Standards.* Report for United States Environmental Protection Agency, Water Management Division, San Francisco, CA. 1994.

Hanemann, W. Michael; McCann, Richard. "Economic Impacts on the Northern California Hydropower System" in *Integrated Modeling of Drought and Global Warming, Impacts on Selected California Resources.* National Institute for Global Environmental Change, University of California, Davis. Pages 55-68. March, 1993.

Dumas, Christopher: and Hanemann, W. Michael. "Simulating Impacts on the Sacramento River Fall Run Chinook Salmon Population" in *Integrated Modeling of Drought and Global Warming, Impacts on Selected California Resources.* National Institute for Global Environmental Change, University of California, Davis. Pages 69-95. March, 1993.

Hanemann, W. Michael. "Economic Impact on Ocean Fishing" in *Integrated Modeling of Drought and Global Warming, Impacts on Selected California Resources.* National Institute for Global Environmental Change, University of California, Davis. Pages 107-112. March, 1993.

Green, Gareth; Hanemann, W. Michael; Helfand, Gloria; and Larrivee, Francois. "Economic Valuation of Changes in the Quality of Life." Report for the California Department of Health Services, 1994.

Mitchell, David L; and Hanemann, W. Michael. "Setting Urban Water Rates for Efficiency and Conservation: A Discussion of Issues." Report for the California Urban Water Conservation Council, September-October 1994.

Recent Presentations

"Modeling Recreation Demand in a Multiple Site Framework." Presented at the Association of Environmental and Resource Economists Workshop on Recreation Demand Modeling, Boulder, Colorado, May 16 and 17, 1985.

"Valuing Genetic Resources." Presented at the University of Washington Conference on the Conservation of Genetic Resources, Seattle, Washington, July 17-21, 1985.

"The Evaluation of Environmental Improvements: Whose Benefits Get Counted?" (coauthored with Nancy Bockstael and Ivar Strand). Presented at the Western Agricultural Economics Association Annual Meeting, Saskatoon, Canada, July 7-9, 1985.

"Economic Techniques for Measuring Environmental Damages." Invited lecture at the California Department of Fish and Game Conference on Environmental Damage Assessment, Monterey, California, October 9, 1985.

"Environmental Protection: Recovery, Dynamics, and Benefit Evaluation" (coauthored with Anthony C. Fisher). Presented at the Conference on Man's Role in Changing the Global Environment, Venice, Italy, October 21-26, 1985.

"The Hysteresis Phenomenon and Benefit Evaluation for Pollution Control in Aquatic Ecosystems" (coauthored with Anthony C. Fisher). Presented at the American Economics Association Annual Meeting, New York, December 27-29, 1985.

"Adjusting for Sampling Effects in Demand Analysis with Single and Multiple Sites." Invited lectures at the W-133 Annual Meeting, San Diego, California, February 19-27, 1986.

"Implications of Biometrics for the Design of Dichotomous Choice Contingent Valuation Experiments." Invited lectures at the W-133 Annual Meeting, San Diego, California, February 19-27, 1986.

"Economic Analysis of Drainage Problems." Presented at the University of California Conference on Decision Criteria for Residuals Management in Agriculture, Davis, California, April 17 and 18, 1986.

Panelist, Resources for the Future Conference on Agriculture and the Environment, Washington, D. C., April 20-22, 1986.

"Pollution Control in a Dynamic Setting." Presented at the Association of Environmental and Resource Economists Workshop on Marine Pollution and the Environmental Damage Assessment, Kingston, Rhode Island, June 5-7, 1986.

Panelist, National Marine Fisheries Service, Workshop on Recreational Fishing Data Needs, Portland, Oregon, June 18, 1986.

"On the Integration of Some Common Demand Systems" (coauthored with Jeffrey La France). Presented at the Western Economics Association Annual Meeting, San Francisco, California, July 1-3, 1986.

Chair, Economics Panel. National Academy of Science/Smithsonian Institution, National Forum on Biodiversity, Washington, D. C., September 21-24, 1986.

"WTP and WTA as Benefit Measures—How Do They Differ? How Can They Be Measured?" Presented at the Workshop on Economic Nonmarket Valuation, Monterey, California, November 14 and 15, 1986.

"Weak Separability, Partial Demand System, and Exact Consumer's Surplus" (coauthored with Edward R. Morey). Presented at the American Economics Association Annual Meeting, New Orleans, Louisiana, December 27-29, 1986.

"Some Simple Techniques for Valuing Improvements in Sports Fishing." Presented at W-133 Annual Meeting, El Paso, Texas, February 11-13, 1987.

"Institutional Problems of Production Agriculture Related to the Regulatory Process." Presented at Workshop on Water Quality in Agriculture, California State University, Fresno, March 20, 1987.

"Discrete Response Contingent Valuation Experiments." Presented at the Economics Department, Vanderbilt University, April 3, 1987.

"Welfare Evaluations in Discrete/Continuous Choice Models." Invited lectures at the Economics Department and College of Natural Resources, University of Michigan, Ann Arbor, April 20 and 21, 1987.

"Current Water Policy Issues in California." Invited lectures at the Economics Department and College of Natural Resources, University of Michigan, Ann Arbor, April 20 and 21, 1987.

"The Dual Structure of Incomplete Demand Systems" (coauthored with Jeffrey T. La France). Presented at the Operations Research Society Meetings, New Orleans, Louisiana, May 4-6, 1987.

"The Structure of Economic Benefit Measures." Invited lecture at the Economics Department, University of California at San Diego, June 10, 1987.

"The Methodology of Discrete Response Contingent Valuation Experiments." Presented at the Western Economics Association Meetings, Vancouver, British Columbia, July 9, 1987.

Invited panelist, Fisheries Canada Workshop on Recreational Fisheries Evaluation, Vancouver, British Columbia, July 10, 1987.

"Biological Uncertainty and the Analysis of Pollution Control Policy." Presented at International Society for Ecological Modeling Symposium on Ecological Economics, Ohio State University, August 12, 1987.

"Economic Valuation of Natural Environments." Invited course of lectures, Economics Department, University of Oslo, Norway, September 3-15, 1987.

"A Generalized Logit Model of Sportfishing in Alaska with Economic Welfare Measures." (coauthored with Richard T. Carson). Presented at the Department of Forest Economics, Swedish

University of Agricultural Sciences, Umea, September 22, 1987, and Stockholm School of Economics, September 26, 1987.

"Beyond Contingent Valuation: Deriving Environmental Benefits from Hypothetical Behavior Data." Presented at the American Public Policy Association Meeting, Washington, D. C., October 29, 1987.

"Economics of Option Value." Presented at Annual Meeting of Society for Economic Dynamics and Control, Phoenix, March 8, 1988.

Invited panelist, "Review of Workshop." Association of Environmental & Resource Economists Workshop on Sportfisheries Valuation, Seattle, June 23-24, 1988.

Invited discussant, Resources for the Future Conference on Natural Resources Damage Assessment. Washington, D. C., June 15-16, 1988.

Invited discussant, Western Economic Association Annual Meeting, Session on Issues in Fisheries Economics, Los Angeles, July 3, 1988.

Invited discussant, University of Maryland Symposium on Commercial Agriculture and Resource Policy, Baltimore, May 4-5, 1989.

"The Supply Effects of Meeting Water Quality Standards." Invited paper presented at Western Economic Association Annual Meeting, South Lake Tahoe, June 20, 1989.

"A Nested Logit Model of Recreational Fishing Demand in Alaska" (coauthored with Richard T. Carson and Thomas Wegge). Presented at Western Economic Association Annual Meeting, South Lake Tahoe, June 22, 1989.

"Zonal Travel Demand Modelling Revisited." Invited paper presented at Western Agricultural Economics Association Annual Meeting, Coeur d'Alene, July 12, 1989.

"The Role of Water Conservation in Agricultural Drainage Control." (coauthored with Erik Lichtenberg and David Zilberman). Presented at American Agricultural Economics Association Annual Meeting, Baton Rouge, August 1, 1989.

"Myopia and the Management of Stochastic Natural Resources in the Face of Alternative Hypotheses." Presented at Oslo Center for Applied Research Conference on Stochastic Models and Option Prices: Applications to Resources, Environment and Investment Problems, Loen, Norway, August 28-30, 1989.

Invited panelist, "Review of Workshop." Alaska Department of Fish & Game Wildlife Valuation Workshop, Denali/Anchorage, September 9-15, 1989.

"Multicomponent Valuation: Valuing the Natural Environment as a Multi-Dimensional Commodity." Presented at seminars in the Department of Economics, University of Illinois at Urbana, November 15, 1989, and the Rand Corporation, Santa Monica, December 1, 1989.

"Review of SOS/T Report 27: Methods for Valuing Acidic Deposition and Air Pollution Effects." Invited paper presented at NAPAP International Conference-Acidic Deposition: State of

Science and Technology, Hilton Head Island, February 14, 1990.

Invited participant, Rockefeller Foundation Seminar on Advances in the Economics of Valuation and Environmental Control, Bellagio, Italy, April 9-11, 1990.

"The Economics of Glen Canyon Dam." Presented at National Academy of Sciences Conference on the Colorado River, Santa Fe, New Mexico, May 24-26, 1990.

"Empirical Analysis in the Use of Contingent Valuation." Presented at American Agricultural Economics Association Learning Workshop, "Nonmarket Valuation: Extending the Frontiers and New Applications," Vancouver, August 7, 1990.

"Valuation of Tropical Rain Forests" (coauthored with Anthony C. Fisher). Presented at United Nations University WIDER Conference on The Environment and Emerging Developing Issues, Helsinki, September 3-9, 1990, and the American Economics Association Annual Meeting, Washington, D.C. December 27-29, 1990.

"Economic Aspects of Natural Resource Damage Assessment." Presented at National Association of Attorneys General Advanced CERCLA/RCRA Enforcement Seminar, Washington, D.C., September 24-26, 1990.

"The Theory of Measuring the Benefits of Quality Change." Presented at the W-133 Annual Meeting, Monterey, CA February 27, 1991.

Panelist, Colloquium on Water Quality Modelling -- Science, Environmental Management and Policy. Presented by the San Francisco Estuary Project at U.C. Berkeley on April 9, 1991.

"The Economics of Loss Aversion." Presented at the Department of Economics & Business, North Carolina State University, Raleigh, May 16, 1991

"The Economics of Environmental Regulation." Presented at 1991 Program of the Tahoe-Baikal Institute, Tahoe City, August 2, 1991.

Discussant, Peder Sather Symposium on Global Climate Change: European and American Policy Responses. UC Berkeley, October 16-18, 1991.

Invited Participant, NOAA/EPA Workshop on Environmental Benefits Transfer and Nonmarket Values Database. Washington, D.C., November 14, 1991.

"The Benefits and Costs of Irrigation Delivery Flexibility." (co-authored with Larry Dale) Presented at US Committee of Irrigation & Drainage Biennial Meeting, San Francisco, December 10, 1991.

"On the Nature of Compensable Value in a Natural Resource Damage Assessment." (coauthored with Richard T. Carson and Nicholas E. Flores) Presented at the American Economic Association Annual Meeting, New Orleans, January 3, 1992.

"The Meaning of Existence Value." Presented at the W-133 Annual Meeting, South Lake Tahoe, February 27, 1992

"Assessing Climate Change Risks: Valuation of Effects." (co-authored with Anthony C. Fisher)

Presented at the Resources for the Future, Inc. workshop on "Assessing Climate Change Risks", Washington, DC, March 22-23, 1992.

"Survey Data Collection: Detecting and Correcting for Biases in Responses to Mail and Telephone Surveys." (co-authored with Barbara J. Kanninen and David J. Chapman) Presented at the US Bureau of the Census 1992 Annual Research Conference, Arlington, VA March 22-25, 1992.

"Critique of the Exxon Papers on CV." (co-authored with Richard Carson). Presented at the AAEA Annual Meeting, Baltimore MD, August 10, 1992.

"Using Economic Incentives to Regulate Pollution: What Do Economists Know?" Invited keynote address at Workshop on Environmental Systems and Economics, Society for Mathematics, Economics & Operations Research 17th Symposium on Operations Research, Universitat der Bundeswehr, Hamburg, Germany, August 25-26, 1992. Also presented at USDA-ERS Staff Seminar, Washington, DC, December 17th, 1992.

"Recent Experience with Natural Resource Damage Assessment" and "Needs for Ongoing Model Development." Invited presentations at North Carolina State University Natural Resources Damage Assessment Workshop, Raleigh, NC, September 18, 1992.

"Lessons from the Natural Resources Damage Assessment," presentation at North Carolina State University, Resources and Environmental Economics Program, Workshop 1992.

"Valuing Effects of Change in Harvest: Marine Recreational Fisheries" and "Implementing Valuation Estimates in Allocation Decisions." Invited presentations at North Carolina Marine Fisheries Economics Summit, Beaufort, NC, September 19, 1992.

"The Valuation of Natural Resources: The Aftermath of the *Exxon Valdez*." Invited Lecture in Distinguished Speaker Series, University of Michigan School of Natural Resources and the Environment, Ann Arbor MI, October 26, 1992.

Member, Panel on "Economic Benefits of Environmental Protection: How Can They Be Quantified?", Conference on California Water Policy: Resolving Critical Controversies, Los Angeles, November 20-21, 1992.

Invited Participant, EPRI/NOAA Workshop on Coastal Impacts of Climate Change, Washington DC, December 15-16, 1992.

"Emissions Taxes and Markets: What Do We Really Know?" Seminar presentation, USDA Economic Research Service, Washington DC, December 17, 1992

"Non-Market Valuation Using Contingent Behavior: Model Specification and Consistency Tests." (co-authored with David Chapman and Barbara Kanninen) Presented at the American Economic Association Annual Meeting, Anaheim CA, January 6, 1993

"Correlated Discrete-Response Contingent Valuation." (co-authored with David Chapman) Presented at the W-133 Annual Meeting, Santa Fe NM, March 15, 1993.

"Current Issues in Dichotomous Choice Contingent Valuation Experiments." (co-authored with Bengt Kriström). Presented at the Stockholm School of Economics Conference on Environmental

Economics, Ulvon, Sweden, June 12, 1993.

"Financial Incentives for Conservation." Presentation to POWER Conference on Restoration Partnerships Under the New Water Law, Oakland CA, June 18, 1993.

"Marginal Cost Pricing and the New LADWP Water Rates." Presented at the Western Economics Association Annual Meeting, South Lake Tahoe NV, June 21-24, 1993.

"Natural Resource Damage Assessment: Economic Implications for Fisheries Management." (co-authored with Ivar Strand). Presented at the Agricultural Economics Association Annual Meeting, Orlando FL, August 2, 1993.

"Water Markets - A Skeptical View." Presentation to UC Berkeley Conference on Regional Water Management, Berkeley Marina Marriott, October 16, 1993.

"The Economics of Mono Lake." Presented at Stanford University Environmental Economics Seminar, November 9, 1993.

"An Appraisal of Contingent Valuation Techniques." MIT Energy Policy Workshop, Cambridge, MA November 18, 1993.

"Pricing Water." Presentation to University of Washington Law School Conference on Water Rights in the 21st Century, Olympia WA, January 29, 1994.

"Learning to Live with Stated Preference." Invited presentation, session on Valuing the Environment: Where Do We Stand? American Association for the Advancement of Science Annual Meeting, San Francisco, CA, February 19, 1994.

"What are the Economic Costs of Biodiversity Conservation and Who Should Bear Them?" Presented at the First North American Conference on Social and Economic Incentives for the Conservation of Biodiversity and Biological Resources. Mexico City, March 9, 1994.

"Problems of Applying Contingent Valuation Methods." Invited presentation at Conference on Environmental Valuation in Context organized for the UK Department of the Environment by the University of Newcastle Centre for Rural Economy, Durham England, March 25, 1994.

"Referendum Contingent Valuation: How Many Bounds Are Enough?," presented with Joseph Cooper at the annual AAEA meeting, San Diego, Calif., August, 1994.

"A Clear Water Policy for the Delta or Just More Mucking About?" presented at the Sacramento Economics Roundtable, Sacramento, Calif., May 24, 1995.

"Nonmarket Valuation under Preference Uncertainty: Econometric Models and Estimation," paper presented with Bengt Kriström, and Chuan-Zhong Li at the Meeting of the European Association of Environmental Economics, June, 1995.